THE SECOND LACUS FORUM 1975

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1975

Edenda curant

ADAM MAKKAI
Executive Director and Director of Publications

&

VALERIE BECKER MAKKAI
Secretary-Treasurer and Co-Ordinating Editor

THE SECOND LACUS FORUM 1975

edited by Peter A. Reich

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PREFACE

The second annual Forum of LACUS, the *Linguistic Association of Canada and the United States,* was held at the University of Toronto, August 3rd through August 7th, 1975. At this meeting fifty papers were read, selected by a review committee consisting of linguistics scholars representing more than half a dozen campuses on the basis of abstracts submitted in advance. In addition, three outstanding scholars were invited to give special evening papers. This year this honor went to H. A. Gleason, Jr., University of Toronto, A. W. Read, Columbia University, and C. F. Hockett, Cornell University. An editorial committee, consisting of the Association's Director of Publications, Adam Makkai, and the Secretary-Treasurer and Co-Ordinating Editor, Valerie Becker Makkai and myself put the manuscripts through a second selection procedure, so that a number of additional papers were regretfully rejected at this stage for a variety of technical reasons. On the other hand, we included several papers which were not presented at the Forum because the authors were unable to attend.

The mood of the Second *LACUS FORUM* was set by Gleason's keynote address, "Continuity in Linguistics", which was warmly received. Another high point of the session was a Chinese banquet honoring Charles F. Hockett. Given the nature of the occasion, Hockett discarded his prepared speech (printed in this volume), and instead talked on "F Phonologies and Cereal Diets". In his talk he pointed out that as far as he knew, no language contained labio-dentals whose people had not developed agriculture. This, he argued, was because their food was tougher, and as a result their teeth did not develop the overbite characteristic of people who grow cereal grains and thus eat a lot of pap. While this was certainly food for thought, it is too bad Hockett did not have a chance to read his other paper as well. His provocative ideas would surely have lead to some interesting discussion.

What was most heartening for me about this conference was the interaction among members. Many papers referred to other LACUS papers, both this year's and last, and the discussion was often spirited and interesting. Especially interesting from my point of view were the many papers in phonology and sociolinguistics. After such a successful conference, one cannot help but look forward to the next meeting in El Paso—Ciudad Juarez.

The organization of papers in a volume such as this inevitably shows the prejudices of the editor. The reader will find this volume organized not by linguistic theory, but by the area of linguistic data dealt with. He will find tagmemic, stratificational, transformational, and purely descriptive papers side by side. My prejudices show through in defining the boundaries of the content areas as well. The reader will note that papers on sign language and graphonomy fall in the section of phonology, for example, because they cover topics which I perceive to be directly comparable to those in phonology. Several papers could easily be placed in more than one section. Virginia Hymes' paper, for example, is both descriptive sociolinguistics and a discussion of problems in field methods. In any case the reader should find interesting papers in all sections and should not be sidetracked if his prejudices differ from my own.

Finally, I wish to thank my secretary and editorial assistant, Nadia Gaichuk, for her long and patient effort on this book, and the other members of the editorial committee, Adam and Valerie Makkai for their efforts and support of my work in putting together this volume.

Peter A. Reich

Toronto, Ontario, Canada
November 15, 1975

I

ON DOING LINGUISTICS: SOME DESCRIPTIONS AND SUGGESTIONS

CONTINUITY IN LINGUISTICS

H. A. Gleason, Jr.
University of Toronto

In the half decade beginning at 1957 American linguistics went through what has commonly been labelled a "revolution". It was not unprecedented, and it will not be the last. We linguists customarily pay passing respects to Pāṇini and then start our history with the revolution allegedly triggered by Sir William Jones. The next major event was the Neogrammarian Revolution. Then it used to be the revolutionary emergence of "Modern Linguistics" in the decade around 1925 and 1933. For many that event has been demoted and replaced by one occurring three decades later.

To listen to the current folklore, 1957 and its aftermath were not merely a turning point, but either a sweeping new start or a return to the lost Eden of Port Royale. Bloomfieldianism had fallen apart from its own internal fatuousness. A science appeared in its place. What happened has been described as "the greatest intellectual revolution since Copernicus".

There has been a great deal of recent discussion of "scientific revolutions". I do not wish to pursue that subject, but rather to speak of its converse, continuity. For, if you take a broad view, you will see that none of these "revolutions" ever made as clean a sweep as its partisans would like to claim. Linguistics has had a remarkable continuity through each of them, a continuity that is, perhaps, more significant than the changes.

Maturity in our discipline will necessitate understanding and valuing that continuity as much as the revolutions – which means coming to a better understanding of linguistics as a whole – and becoming as much concerned for the future continuity as we now are for theoretic debate and polemic.

The common view, that the history of linguistics is a series of revolutions in theory coming at shorter and shorter intervals up to the final revolution – ours – after which only the details have to be filled out, results in part from seeing linguistics as more or less equivalent to linguistic theory, and in part from seeing linguistic theories as either true or false. Linguistics (or any other discipline, for that matter) is much more than theory, just as a theory is not at all what we commonly assume it to be. One way of looking at our discipline is in terms of four components: theory, heuristics, results, and applications. Though there are other taxonomies equally worth attention,[1] it is this one that I want to start from, and it is this that I want ultimately to modify. I am assuming that a discipline not only comprehends these four, but in some significant sense is characterized by how these four are regarded and how they are made to interact.

In a simple, superficial way we can see that each of these four has its proper part to play: Theory is how we keep our intellectual house in order. Heuristics is how we go about our work. Results is what we have to offer. Applications is how we justify our place in society. All are essential, not merely as part of the practice of linguistics, but as part of the content of linguistics.

I hardly need argue that the four are not equally respected today, either as practice or as content. The pecking order puts theory and theoreticians at the top, and applications and their practicioners far below. Leading theoreticians have said in public addresses something to the effect that linguistics does not have applications, and that, anyway, if it did, they personally would not be interested in them. I need not document any further our sorry state here. Every linguist is aware of it – too many, unfortunately, with either satisfaction or complacency.

Before I discuss continuity in these terms, I must identify two of these key components more precisely. Since I shall base my discussion on recent Northamerican linguistics, that

[1] For example, it is equally worthwhile to look at linguistics and similar disciplines in terms of content, practice, ethos, and organization. Indeed, such an examination is complementary to that made here.

means on Bloomfieldianism and its descedents, principally Transformationalism, Stratificationalism, and Tagmemics.[2]

One of the recently favorite ways of damning Bloomfieldians is to allege that they were devoted to developing "discovery procedures",[3] the reductio ad absurdum of heuristics. This charge reveals obliquely not only contempt for heuristics, but also a complete misunderstanding of Bloomfieldianism.

1. Theory and Heuristics in Bloomfieldianism

What then was the picture? Bloomfieldians had a dislike for the word *theory*. It tended to suggest all that they thought wrong-headed or even disasterous about European linguistics of their time. They tended to refrain from saying *theory* in favourable contexts. And they refrained from discussing language in a framework reminiscent of what they knew as *theory*. Instead they proposed and discussed *procedures*. For the most part they made no claim that these *procedures* could be applied in an actual analytic situation. Occasionally they made this clear and explicit by speaking of *idealized procedures*.[4] For practical purposes they had recourse to *short cuts*, generally considered as at a distinctly lesser level of validity and interest. The contrast between *short cut* and *procedure* is quite comparable to that between *discovery procedure* and *theory* in the language of their latter-day critics. The best available contemporary one-word glosses are 'theory' for *procedure* and 'heuristics' for *short cut*.

For example, one *procedure* that was discussed would segment an utterance into morphs by use of transitional probabilities between the successive phones. To actually do this would involve, first, counting higher order frequencies for perhaps millions of words of text. This was before computers, but even today such counts would be formidable and expensive. No one ever tried this – except in a sort of half-hearted simulation using the judgements of native speakers in lieu of the statistical information called for, and then only on very short samples. This could not be considerd a serious *test* of the procedure. At best it could be compared with a free-hand diagram on the black-board in a geometry class; it illustrated, but it did not prove, nor even demonstrate. Segmenting by transitional probabilities was not suggested as a heuristic technique. The discussion was about theory.

Note that Bloomfieldians *debated* procedures; they did not *test* them. That is a fairly sure mark of the difference between theory and heuristics. For example, I have heard – and participated in – hours of debate on one of the stock questions of the day: "Can grammatical analysis be done without recourse to meaning?" That was one way of asking about the theoretic

[2] The current folk-lore, since it is committed to viewing the history of linguistics in terms of revolutions, tends to minimize or deny the relationship of, at least, its favoured school to Bloomfieldianism. At the same time one way of putting down some alternative school is to assert that it is only thinly disguised Bloomfieldianism. It is probably more profitable to view the four named as a philogenetically related family of theories. Each is, of course, a genus of theories rather than a specific theory. In any case, much of what I have to say applies to all of these alike, though not necessarily to some other schools of different history and largely of non-Northamerican provenience. As I am addressing the Linguistic Association of *Canada and the United States*, I need make no apology for some provincialism.

[3] "Discovery procedure" is not a Bloomfieldian term. It was introduced into the literature, so far as I know, only in the anti-Bloomfieldian polemic.

[4] Bloomfieldianism was not, of course, a homogeneous position and no brief characterization is quite valid. For some *idealized procedures* could only apply in unattainable ideal conditions. For others they were thought of rather as being applicable in some possible future when knowledge and skills would be far advanced. For very few were they real options in the present or immediate future. To speak in terms of procedures was, of course, an elaborate sort of metaphor, just as it is to speak in terms of rewrite rules. Then as now some linguists took the metaphors too literally, but to assume that Bloomfieldians accepted them in the way commonly ascribed to them is comparable to assuming that users of rewrite rules visualize the brain as equipped with chalkboards and manned by little demons with chalk and eraser. I have never met a generativist with such notions, nor a Bloomfieldian who took the discussed procedures completely literally. But I have met a number of linguists of these and other persuasions who were far more literal than the leading thinkers with whom they claim to agree.

standing of meaning, or about the place of semantics in linguistics, or about the nature of the morpheme, or a number of other closely related matters. It touches on a number of questions that are still very much alive today,[5] even if they are never posed in the same way. The debate was vigorous, elaborate, and long-continuing. But, notice, it was always *debate*.

I know of only two linguists who experimented with anything remotely like this, William E. Welmers and myself. In one of my experiments I had a student who was a competent speaker of a Mande language from West Africa and a Papuan language prepare for me a corpus of one glossed in the other (or vice versa), both totally unknown to me,[6] and without even a hint of meaning accessible to me. With this I started to analyze both languages. I had gotten a good start, too, when the informant's turn came up in a departmental seminar, and I could no longer maintain my innocence of one of the two languages. But I had done enough to convince myself (and the student-informant) that I could have gone a fair distance into the grammars – perhaps not all the way, but one never makes such demands of a heuristics.

I did not conceive this experiment as dealing with the commonly debated question[7] – indeed, I stood fairly far toward the pro-meaning end of the range in that debate. Rather it would, I hoped, lead to some insights that would be practically helpful, that is, to some insights about heuristics. And for me it has. There is no inconsistency here. I had through the fifties the strange conviction that we should avoid the use of meaning as much as possible in the earliest stages of grammatical investigation, saving it for later when we would need it more desperately. That may have been in part a misconception arising from the still too common confusion of meaning and glosses. But in any case, heuristic techniques over and over again show strange non-conformities with theory, even when they are superficially similar. That is one reason for keeping them apart in our thinking and in a discussion such as this.

I said I only *knew of* two linguists who tried anything like this. I have no guarantee that there were not many more. The more there were, however, the stronger my case. Any unknown experimenters underline my point that even those in the Bloomfieldian era who did investigate heuristic matters seldom made public confession of their behaviour. There was no eager audience for discussions of heuristics.

If you will accept 'theory' as the proper gloss for Bloomfieldian *procedure* and 'heuristics' for *short cut*, and then look at the record, you will find that the Bloomfieldians were not, in fact, anti-theoretic. They enjoyed theoretic debate, conducted in their own peculiar medium of discussion. It was theoretic debate that animated the programmes of the L S A, and some of them certainly were animated!

But they were ambivalent about heuristics, with a very strong strain of disinterest. When they mentioned *short cuts* at all, it tended to be with an apology for bringing the matter up. Such things were seldom formally discussed in the L S A meetings, and hardly at all in the pages of *Language*. Linguists did not adopt the psychologist's convention of devoting half a paper to specifying methods; instead they quoted Bloomfield's dictum: "When you have guests to dinner you don't have to take them into the kitchen." In elementary textbooks and lectures it was permissible to discuss such things – those were directed to beginners.

The strongest bit of seeming counter-evidence is in the standard, conventional graduate programme. The core of the curriculum was three courses in a fixed sequence: phonology, morphology, and field methods – the actual labels varied slightly. Don't be fooled by occasional

[5] The closest parallel is probably the recent debate between interpretive and generative semantics.

[6] It was crucial to the success of the experiment that they be typologically disparate. In this respect the operation performed was quite different from familar use of glosses, where one common technique is to juggle the glosses to bring them into typological conformity with the language under analysis.

[7] At least not directly. Of course, what is learned about the success or failure of heuristic techniques is data that should be taken account of in theorizing. Heuristics work, when they work, and fail, when they fail, because of certain peculiarities of language. But such evidence is oblique. I suspect that I saw the relationship as somewhat more direct then than I do today. But I know that I was then quite aware of the distinction between theory and heuristics, as were most of my colleagues.

titles like *Phonemic Analysis*. That term *analysis* could be a synonym for *procedure*, and so might really mean something close to 'theory', at least in large part. In any case the centre of attention in the first two courses was more often theory than heuristics. Heuristics was included; it had to be taught, at least the rudiments, even if linguists preferred not to talk about it much except to beginners. Indeed, the Bloomfieldians let Kenneth L. Pike and Eugene A. Nida write the only real textbooks for these courses. They were marginal characters who investigated languages for Bible translating and other utilitarian purposes; they were willing to look seriously at the problems. The profession thanked them, when it thought of it, in the same way we always thank the menial labourers who do the necessary but uninteresting jobs. All the while, in class and out, "serious linguists" sniped at the superficial and shallow practicality of the books.[8]

Field methods courses could not so easily slide over into theory. The class was typically confronted with an informant, and, with an amount of guidance that varied from instructor to instructor, had to produce some part of a description of the language. One pattern left the students pretty much on their own, sink or swim. (Some of them sank without either the student or the professor knowing it!) Another pattern was to spend much of the class time in discussion – generally not of techniques, but of interesting structures in the language. Some instructors merely conducted their own research in front of the scarcely comprehending class. But very few instructors discussed very deeply, very extensively, or even moderately insightfully, the problems of techniques, the problems of human relations with informants, the relation between field work and language on one hand and field work and linguistics on the other, or much of anything that could be considered direct facing of heuristic questions.

Field work itself was a rite of passage. After a brief and generally unrealistic preparation, the initiand was banished for a summer from academia and its comforts. On his return he performed certain tribal exercises related to his experience, grilling by the previously initiated being a prominent part always. He emerged, if he survived, legally an adult linguist.

It is easy to understand why neglect of, or bias against, heuristics would arise and how it would be perpetuated and deepened in any school dedicated to Bloomfield's great objective of making linguistics a science.[9] Heuristics is a collection of disconnected, disparate, unreliable techniques. There is nothing to appeal to the systematizer, nor anything to give him scope to work. In any case, in Bloomfieldianism (and in most of its successors) heuristics has had a marginal place in the practice but little or none in the content.

2. Heuristics

It is characteristic of heuristic techniques that they inevitably fail. Chomsky was not the first to observe this, nor the first to be put off by it. It is not merely that heuristic techniques do not automatically reach their goal. Rather, used even with great care and meticulous monitoring, they still fail. I have heard heuristics defined as something that you do that you know won't work.

However, they do not all fail in the same way, nor on the same material, nor at the same place in the task. The practical linguist's hope is that, when one technique fails, another may

[8] Specifically, I have heard both Pike's *Phonemics* and Nida's *Morphology* criticized for too much attention to *short cuts* and not enough to *procedures*. The concern for applications, even shown in the subtitle of *Phonemics, A Technique for Reducing Languages to Writing*, was, of course, even more serious cause for scorn.

[9] This goal is not new in linguistics, reaching back as far as the current prestige of science. But there have been few linguists who urged it more constantly or more insistently than Leonard Bloomfield. Of course, notions of just what constitutes a science and what would be necessary for linguistics to qualify as such have changed over the decades, in linguistics, in other sciences, and in the popular view. These changes are interrelated, but it does not follow that the views of science held in different disciplines at any given time are identical, or even simply related. For that matter, they have seldom been uniform within any one discipline. A thorough investigation of this theme through the history of linguistics is urgently needed and would certainly profoundly alter the common understandings of linguists.

take over and carry the task forward a bit farther before it too fails. It is therefore desirable to have as many different heuristic techniques available as possible. In this way the probability is increased that a useable one can be found. But we have been reluctant to pool our experience, and still more reluctant to investigate and discuss the strengths and limitations of specific techniques. That is, we have of necessity accepted heuristics as a part of linguistic practice, but refused it a place in linguistic content. As a result practice has suffered as our choice has been narrowed, and our bases for informed choice have been restricted.

This has been unfortunate. Heuristic techniques fail in various ways, sometimes relatively harmless and sometimes very dangerous; sometimes easily detected and sometimes quite subtle. They may fail in not producing results, or they may fail in producing false results – occasionally in quanity. May I illustrate with two strongly contrasting cases:

You will find in the literature a number of papers in which relationship is "established" between language groups by random searching through vocabulary lists for items that are resemblant in form and meaning. In some this is followed up by various checks, some of them very rigorous. In others no further support has been sought. In these unchecked instances, when the relationship found is one that linguists generally want to believe, no question is raised. When it is not, we generally merely label the investigator as "careless". But the fact is that the technique used has the most dangerous characteristic possible: The less relevant and satisfactory the input, the more likely is the technique to produce convincing looking results. The method will prove anything, provided only that the input is poor enough!

In contrast, consider the use of glosses in grammatical investigation. We usually evaluate this, if we think about it at all, in terms of how well glosses convey meaning. We know they are terrible. We conclude that the use of glosses is unjustifiable. But we use them anyway, either because we know no other way, or because our experience contradicts our intellectual convictions.

And it should! Our conventional wisdom about glosses is largely irrelevant or wrong. Glosses have at least three distinct and important functions: to suggest meaning, to aid in selecting items for comparison, and to give access in reëlicitation. Of these, it is the second that figures in early morphologic analysis. We select from our corpus pairs or sets of forms that, among other things, share some feature in their glosses. For that purpose it does not matter whether they convey meaning accurately or not – or whether they convey meaning at all. That I have proved to many students by having them work with a corpus glossed only in Sango.

Glosses do, of course, fail us – commonly – but the possibility of really serious damage arising from glosses is very low – with a proviso to which I will return. They might fail us in either of two ways: They might fail to bring together items that should be compared. This is fairly frequent; as a result the work is not carried forward as expeditiously as it might be. Or, they may bring together and cause us to compare items that should not be. This is actually very rare; the consequence is that they very seldom contribute to false results. They fail in a highly safe way.

I said that there is a proviso. It is crucial, in fact. What I have claimed about the safety of glosses applies to those on open-set (vocabulary) items; it does not apply to glosses on certain kinds of closed-set items. Glosses are not all of equal utility or safety. There are, however, rules of thumb which suggest how much reliance one can put on glosses in any specific case or specific use.

I doubt that most linguists know much of these peculiarities of glosses, or of their consequences for their work. I am sure that a great deal of further investigation in this area would be worthwhile. It might come to pass if critical discussion of heuristics were socially acceptable in linguistics – if heuristics were recognized as having a place, however minor, within the content of the discipline.

A second fact about heuristics is little understood. Heuristic techniques are highly theory-independent. There is no such thing as a TG heuristics, nor a stratificational heuristics, except by the fortuitous association that arises by the way techniques are transmitted from professor

to student rather than by wide diffusion through the profession, and except that the independence of heuristics from theory is occasionally obscured or confused when the distinction is blurred or distorted.

We have noted how the Bloomfieldian expressed theory in terms of *idealized procedures*. This does lead to some admixture of heuristic considerations in their theory, or of unwarranted theoretic constraints on their actual working practices. But this is minor compared with the consequences of leaving heuristics outside systematic scrutiny. amd public discussion. In fact, the latter would very likely have corrected any confusion.

The Tagmemicists, however, depart from the Bloomfieldian tradition by deliberately trying to combine theory and heuristics into one structure. This has imposed severe limitations on their theory. But it has rehabilitated heuristics as, for them, a discussable topic. It has thus had both positive and negative value, but the positive only because it has helped them avoid the unhealthy relationship between theory and heuristics that prevails in the attitudes of Northamerican linguists.

Transformational Generativism seems to have deepened the anti-heuristic bias it inherited. But we must not be misled. A necessary minimum of heuristics has been preserved, partly kept out of sight like the Bloomfieldian *short cuts*, partly made respectable by incorporating it into theory. If you consider theory everything, then only these two courses are available to you. The prime example of the latter recourse is the treatment of "intuition". Its use is not new with T G. Bloomfield, for example, described his own phonemic system; one wonders how he could have done so while scrupulously avoiding introspection. Chomsky brought the use of "intuition" into the open and made it respectable, by the only device open to him: he made it part of his theory.[10] This has been unfortunate for two reasons: First, there was a confusion between intuition about language and intuition about intuition about language, and this has major theoretic consequences. Second, theoretic status for a heuristic technique has tended to guarantee a validity that it cannot deserve, and this has major practical consequences.

Introspection, as I would prefer to call it, is a valuable tool, but one that carries very grave dangers. Its characteristic method of failure is false results and high confidence in them. It must, therefore, be used with explicit precautions. Careful workers know this in their bones. But linguistics would be advanced by making the problem explicit and subject to investigation.

But note, if intuition is misused – and I think it often is – this is no peculiar fault of the Transformationalists, but only the specific consequence within their framework of an anti-heuristic bias that pervades Bloomfieldianism and most of its descendents. It finds different expression and different ways of rendering its damage within other schools.[11]

[10] Universals – particularly substantive universals – can be looked on as a quite parallel matter. The value of the accumulated body of results is recognized, but linguistics is effectively identified with theory. The only way to give significant recognition to the perceived value of results is to incorparate them into the theory, and this effectively requires that they be treated as universals because the interest is in a theory of *language*, not of specific single languages. The roster expands rapidly because every fact of interest must be a universal to justify that interest.

The question whether a given fact is or is not a universal might be an empiric one – I would like it to be such – and indeed it is usually claimed by proponents of universals that such questions are empiric. But there is another aspect of the matter that complicates the problem. The notion of universals has become largely a principle of relevance determining what data is applicable to a given problem. A strict constructionist would say that only data from the idiom under analysis is relevant. A broad constructionist will admit data from any language. This debate is not new in Northamerican linguistics: The debate in the fifties over the "over-all pattern approach" to English phonemics was a debate as to what range of data is relevant to the analysis of the vowel system of a specific dialect or idiolect. E.g. Does Toronto English have /ɨ/(barred i) because dialects spoken 500 km away have plentiful contrasts like /mɨlk/ : /silk/?

[11] And, for that matter, Transformationalists are not the only ones that have misused introspection. Others are likely to have the difficulties compounded because they are unaware, even, that they use it.

3. Theory

Heuristics, at best, faces the task piece-meal. It is for theory to fit the fragments together and give us a comprehensive picture. It is this integration which gives the linguist his intellectual satisfaction, and hence it is theory that he sees as the essence of linguistics. It ties one detail to another, and in so doing provides a series of checks. It suggests problems. It gives a launch point for sallies into new domains. I need not go on, and I need not go into details. The virtues of theory are familiar today – perhaps over-familiar.

But in an age where *theory* seems almost automatically to mean 'formal theory', or even just 'formalism', we do need to remind ourselves of what a theory really is.

A linguistic theory is any partially successful effort to make broad, systematic, generalizing statements about language. But this can never go very far in ordinary folk-language. Theory formation requires more precise formulation than that allows. One possibility is to develop special vocabulary, tighten up sentence structure, and work out appropriate conventions of statement, that is, to create a special meta-language. It is this meta-language that marks a theory.

In contrast to what we discuss these days, older theories can be called *informal linguistic theories*. Formal theories are diachronically and synchronically rooted in informal theories. The relation may be stated over-simply in this way: Tightening up ordinary language is found to be inadequate, or too slow, or too laborious. Parts of the theory are restated in a new, presumably more satisfactory, medium. Typically this is a mathematics. The old meta-language – for these parts – is reduced in status to become a mere set of glosses on the new.

Notice, I say "a mathematics". There are a multitude from which to choose, each with its own range of properties. Hand-in-hand with the choice of a mathematics goes the choice of a set of principles of mapping, by which language elements and language relationships are equated with mathematical elements and mathematical relationships. If this is done properly, all the properties of the mathematics are preserved, and they can be used in making predictions about language. The combination of a mathematics and a set of mapping relations is a *model*. It replaces some chunk of the old theory, but never all. A *formal theory*, then, is one in which part of the statement is in the form of a model and the remainder is, as before, in a specialized form of natural language. All theories are, in part, at least, informal.

Formalization only carries further what meta-language development has started. It increases the precision of prediction and checking. It makes statement more concise. The gains may be appreciable, but they are never absolute, and they apply, of course, only to those parts of the theory that are formalized.

It is alleged that formalization, also, makes the whole more easily falsifiable. But since more than the formalized part is always involved in falsification,[12] and since theory testing is an extremely involved process, it is not clear whether and to what extent such a claim is even meaningful.

[12] No obervation is automatically evidence. A theory defines, for itself, its own object. That is, any given linguistic theory defines just what is language, and no two necessarily do so alike. Moreover, every theory includes (in many cases only covertly, but this is a defect in its formulation) a set of criteria of relevance by which the evidential value of observations – even within its defined object – must be weighed. Typically these allow minor details of a theory to be falsified more easily than major points, and the latter more easily than the theory as a whole. This is necessary; a theory must be protected from any little chance quirk of speech or misled intuition. The problem is to provide for a theory just that degree of protection that is necessary for it to function at all without sheltering it so completely that it becomes useless in another way. Formalization does, assuredly, facilitate falsification (that is, testing) of formalized details. But there is little reason to believe that it does anything of the kind for the theory as a whole. In the latter case, the criteria of relevance exert a much stronger control than is ordinarily realized. The notion of competence and performance is an example of a theory-protecting device that is almost certainly too strong. It precludes falsification of much of any theory with which it is associated. It is, however, neither a necessary component of T G – as shown by its virtual abandonment by many T G linguists – nor restricted to T G, since it has diffused widely.

Much is claimed for formal theories. Two claims that are particularly enticing, and hence commonly advanced and eagerly accepted, are utterly groundless for any sort of theory, formal or informal, actual or imaginable.

First: No linguistic theory, Transformational, Stratificational, Tagmemic, or what-not, can tell us anything deeply significant about the human mind. The black-box theorem should teach us that. It says that, given only the *paired* inputs and outputs of an inaccessible device, it is impossible to definitively describe that device. In the case of language and mind, we have inputs and we have outputs, but they are not paired. For the interpreting function of language we have only inputs; for the production function of language we have only outputs. Unless we are willing to broaden the domain of our theory to include, among other things, inferencing, memory, and general knowledge of the world, we cannot pair them. So a purely linguistic theory (as we customarily delimit linguistics) must work within constraints much narrower than those that the black-box theorem shows to be absolutely limiting.

This is not to say that the mind is forever out of reach; only that it is necessarily and permanently out of reach to linguists working solely as linguists. Other kinds of data than speech outputs and speech inputs[13] must be sought and fully exploited, and most of those are of kinds outside the competence of linguists. Speech is a clue – perhaps the most important clue, and its precise specification is essential to the entire endeavor. So linguists must continue to have a part, but only within a much broader team that will comprehend at least psychologists of several different kinds, neurologists, pathologists, cognitive anthropologists.[14]

Incidentally, this fact that mind is out of reach to linguists has nothing whatever to do with the arguments between "mentalism" or "rationalism" and "empiricism" or "positivism", or with any of the other possible positions of the same set. The limitation holds in only slightly different form within any of these.

Second: Linguistic theory cannot explain anything really significant – if you have in mind any meaning for "explain" beyond 'fit into a big picture'. It cannot explain in the sense of answering the question "Why?", beyond pushing the question back, step by step, in endless regress. The most a linguistic theory can do is something like this: Given two linguistic facts, *A* and *B*, connected in our theory by some relationship *C*, we can conclude that whatever explains *A* has a certain probability of being connected to whatever explains *B* by some connection relatable to *C*. Any real facing of the question "Why?" requires a recourse to ontology, metaphysics, or theology in some mixture. If you are willing to build such into your theory, then it can say something just a bit more about explanation. But any reasonable scheme will bring you to something far more humble than the linguist's usual claims of "explanatory adequacy".

Rejecting these pretensions of theoretic linguistics, there remain a great many virtues. particularly of carefully elaborated, critically understood, formal theories. But we must face squarely up to the costs. I will mention only three.

First: formalization reduces flexibility at the same time that it reduces looseness. Indeed, looseness and flexibility are merely undesirable and desirable faces of the same thing. Highly formal grammars have approached unmanageability. To a considerable extent, adoption of a formalism locks you in, for better or for worse. Lambograms are no different in this than rewrite rules.

[13] Intuitions about language are not an additional kind of data – or, at least, not greatly different. A speaker's intuition commonly is of the form "I would (not) say *X*", i.e. it is an intuition about what might or might not be a speech output. If such intuitions are immune to certain "performance failures" (e.g., slips of the tongue or phonetic slurring), they are still outputs from the mind. At the best only the most superficial factors are bypassed. Such intuitions are like driving a pipe into a spring; you get the same water with a little less of the dust and leaves that blow into the open pool.

[14] "Broadening" of linguistics to make it psycholinguistics is not enough. The problem goes far beyond that. And the whole is so complex that it will demand team work among specialists, not merely the efforts of generalists, however broad.

Second: No formal model can possibly cover all that is worth saying about human language. If a model carries all the properties of its underlying mathematics, certainly one of these is what Gœdel discovered, namely that there are things true in any mathematical system that cannot be derived within the system. But the limitations are far larger than that. We can never succeed in formalizing all theory. Yet our established working habits force us to favour those parts that are formalized. We tend to act as if those parts not formalized were unimportant, or in some way indisputable common sense, or validated automatically by the parts of the theory that are formalized. None of these alibis makes sense.

The fact is that formalization inevitably tends to narrow vision in some places, just as it may widen it in others. And the narrowing often exceeds the widening, no matter what formalization(s) you are subjecting yourself to.

4. An Excursus on Applications

May I digress a moment to say something about applications, the one of my four components of linguistics that will have to bear the brunt of time limitations. Its obvious value to linguisics is that it justifies our place in the community, whether that be academia or the broader society. But we should never assume that is all it is good for. For one thing, applications are continually turning up neglected problems, ones that we have failed totally to notice, or ones that we have turned our backs on.

Discourse structure is just now coming to be one of the active and productive sub-disciplines in linguistics. The pioneering work was done largely by trained Bible translators, that is, by applied linguists. They found their work running aground repeatedly on a series of problems that their linguistics knew nothing of,problems that would not go away, problems that insistently called for investigation whether the traditions of linguistic theory had a place for them or not. And, as we get into discourse, we are beginning to see that it has the keys to a number of the problems that we have long considered to be within the normal domain of linguistics.

Within computer science departments and computer research laboratories work on the human-machine interface has called our attention insistently to language decoding, a whole area that we linguists had resolutely denied for an academic generation. People likeTerry Winograd and Roger Shank began working on a central problem of linguistics when the majority of linguists were calmly taking refuge in the notion that a generative grammar is neutral as between speaker and hearer, therefore there is no problem, and in the notion that only competence could be interesting, whereas performance questions are intrinsically unsolvable. How much better it might have been, both for computer science and for linguistics, if more of that work had involved linguists. When we neglect applications, we pay – in some instances by the chagrin of finding non-linguists doing our work, and occasionally doing it better, and worse yet, by finding them actually doing what we elected to axiomatize into impossibility.

3b. Resumption of 3. Theory

Third: Every theory – more formal ones more than less formal – produces analytic artefacts. These are of two major kinds: Some notions or results seem thoroughly established in and by the theory, but in fact are ultimately untenable, meaningless, or counterintuitive. Some questions are raised that seem of fundamental importance and warranting close attention, but in actuality are wholly or largely contentless, irrelevant, or misguided. The first gives a feeling of accomplishment, though the basis is fatuous; the second sends us off on long and complex research regimens that in the end prove snipe hunts. My volunteered paper, submitted before I was invited to give this, will constitute an exemplification of how different the results and the problems can be in two frameworks. In each, part of the difference is probably analytic artefact.

Or you can look back at any older theory and find your own examples. And rest assured that in some future generation linguists will be able to look back on yours with the same result. There is no way to identify artefacts within any one theory, and no way to be certain in any case, but we can be certain they exist. We must learn to live with the knowledge that they are there and that they are misleading us.

But we can also move to minimize the costs or compensate for the losses entailed. Two courses seem readily available to this end.

The first flows out of more, and more explicit, investigation than we have had of the nature and limitations of theories in relation to other components of the discipline and to language. We have tended to concentrate, in meta-theoretic work, rather on the nature of formalisms, and that in isolation from other elements of theory or other parts of the discipline.

The second is effective pluralism in formal theories. The artefacts produced by one theory do not match those of another. When results or questions seemingly interesting in one theory appear trivial or irrelevant in another we should be suspicious. We cannot always know which of two theories is misleading us when they seem to conflict, but we can be put on guard. The limitations of horizon are not the same in one theory as another. And working in two theories can occasionally give us a route around some of the stifling inflexibilities.

By pluralism here I mean the shifting back and forth from one theory to another and the continual comparison of one with another, not merely in general discussion, but also in the treatment of specific problems in specific languages. Almost invariably it will be found that each makes a contribution to the problem that the other cannot.

However desirable it might be, it is not necessary for every linguist to be able to work in every formal theory. We are, fortunately, past the stage where it had to be, in most instances, one linguist per language. We can have, for many languages, a plurality of approach in a plurality of investigators. But if there is to be effective interchange, there must be, at least, receptive bilingualism in meta-languages as normative for all linguists, and a readiness to add to the repertoir when necessary.

And we must retreat from the stance which asserts that one's own theory is right, all others wrong, and that mans the battle lines to prove the superiority of a theory by invective, distortion, and the weak-point-pound-like-hell technique.[15]

Theories change, sometimes with surprising suddenness. We like to look on history as a series of "revolutions" with relatively quiet times between – times first of consolidation and then of stagnation setting the stage for the next "revolution". But in those quiet times there is also change, gradual and unobtrusive. Sometimes it is broader and more profound than the sudden and obvious changes that mark the beginning and end of a period. Such change is, of course, more a fact about linguists than about linguistic theories. A full discussion would have to probe deeply into the sociology of the discipline. Linguists over-react to shortcomings in theories. They seem always in search for that faultless theory that they are sure is just coming into popularity.

In any case, some change is more apparent than real, and, conversely, some change that passes scarcely noticed is fairly fundamental. Still, stability and continuity are not obviously to be found in the theorizing of the linguistic theoreticians. We never look for it there in history, but we are confident that it will be found here in some soon-to-dawn future.

5. The Body of Results

In addition to theory, heuristics, and applications, linguistics also includes a body of established results. This is embodied in a vast library of reference works, monographs, dissertations, and journal papers, and of course also – and equally important – in the minds of linguists. In contrast to the twisting and redirection in theory it seems stable. It does, of course, grow constantly by cumulation of new results, and occasionally old "facts" are dropped or restated or revised. But there are no revolutions, no evident breaks, and no proclaimed break-throughs.

We might conclude that here is the centre of continuity, the stabilizing mass, the inertial movement that goes steadily forward while theory gyrates dizzily around it. There is a grain

[15] No one group of linguists has any monopoly on such behaviour, and it is not peculiar to linguistics. Probably no one other factor has damaged the standing of linguistics in academia to a greater extent in recent years.

of truth here. This body of "fact" must have a place in the continuity of the discipline, very likely a central part.

Yet, on a little reflection, some doubt must arise. No linguistic "fact" can be very effectively stated without meta-language, the handmaid of theory. Nor can any collection of "facts" be tied together into a body without something like the structuring that we think of as being central to theory. What, then, is it that holds together and organizes this body?

Joseph Greenberg[16] has made a suggestive observation. He points out the common practice of linguists in first stating a problem, then proceding to give it more theoretic analysis and statement. In this preliminary statement, he says, they use a recently obsolete theory. He cites as an illustration Emmon Bach's stating a phonologic problem in Bloomfieldian terms before launching into a generative treatment.

What Greenberg points out is certainly common enough, and by no means restricted to TG papers. But two things lead me to question his interpretation: *First:* You will find this technique employed by linguists who otherwise give little evidence of understanding of or sympathy with the previous theory, as, for example, by transformationalists who elsewhere seriously misinterpret Bloomfieldianism. *Second:* You will see in such statements, from time to time, terms that are not Bloomfieldian. A fairly common example is *neutralization*, of Prague School provenience (and anathema to orthodox Bloomfieldians). It is always hard to demonstrate an absence but I have the feeling that some Bloomfieldian characteristics are missing.

That is, what Greenberg has labelled as "Bloomfieldian" is only superficially such. It is, if I may put it metaphorically, a sort of pidginized, or perhaps creolized, Bloomfieldianism, full of loans from other than its base, simplified in certain crucial respects, and functioning in a restricted context. (It is used to state problems, never to solve them.) Something much like it, if not identical with it, is the vehicle of common communication among meta-dialectally isolated linguists. As such it has a second very important, even indispensible, function.

This pidgin or creole metalanguage grows and changes in very much the same way that the body of results does. New items are added whenever they are found necessary or useful, from whatever source may be at hand. It is, of course, in current North America largely Bloomfieldian in its base – that is our common history. And it has been basically the same as long as I have been in linguistics, that is since the late forties. In that era it had already been largely relexicalized with common Bloomfieldian terminology – "common", that is, to the numerous sects and tendencies, but still with a strong residue of pre-Bloomfieldian traditionalism.

At the risk of carrying an analogy too far, may I call to your attention that I have vacillated between comparing it to a pidgin and to a creole. What would be the difference? If it is strictly a second meta-language for all its speakers, convention would require comparison with pidgin, but if it is the first for some, with creole. I prefer creole, for it is what indeed *should* be the first meta-language of the infant linguist. This is what should be taught in our introductory courses. This is what our students will need no matter what theoretical bent they are led to. This is what they must have as the currents of latest theoretic fads swirl around them, undercutting one year's learning in the next. This is what will give them maximum access to the stored facts, whether they remain in linguistics or go into some related discipline.

An important part of the organization of this bank of results is carried by typology, the real cinderella of linguistics, performing much menial labour for no recognition. We can think of typology as a non-theoretically based taxonomy of phenomena. But it is more like a theory than this, more than we usually admit. A typology differs from a theory only in that the predictive function is minimized and the taxonomic function emphasized. But for the purposes at hand the predictive function is unneeded and the taxonomic function required – we are concerned primarily with organizing "facts" for effective retrieval.[17]

[16] In a working paper, not to my knowledge yet formally published.

[17] Another reason to regret the denigration of *taxonomy* in recent linguistics.

6. Core Theory

But we are left with questions: How does this apparatus relate to theory, that more elaborate, more volatile machinery to organize and relate? What really is theorizing, that sometimes almost frantic activity that takes so large a part of our effort?

First observe that theorizing feeds on this bank of results. New theories are only rarely and never more than partially based on new data or on direct reöbservation of old data. The theoretician draws rather on the bank of established "fact". It is there, already marshalled for his use, arranged in an order that is tractable, if not precisely what the new theory will itself impose. There can be found in this repository a great store of interesting phenomena against which any theory should be put to the test. Among them will be found the details on those matters which have strained or discomforted past theories. Among them will be found some, at least, of the key pieces that can be used as argument for the new theory. Theorizing not only feeds on this accumulation; it is to a certain extent controlled by it. The absence from the bank, or the inaccessibility within the bank, of a crucial "fact" may restrict or misdirect the process. The right sort of data may stimulate significant development.

As I hinted above, there is a certain element of illusion in the wide swinging of theoretic debate from one position to what seems its polar opposite, and then off in some new direction. Theories do not differ as much from one another as they appear to. Much of that appearance is artefact of our conventions of theorizing. New theories are never totally new. Rather, they are very largely new combinations of old elements, with a new idea or two incorporated. Theorizing, then, is largely an on-going process of experimenting with new combinations of elements, and only secondarily of finding and testing new elements.

So we might think of linguistics as having also a second bank of ideas, a store of theory components. Out of this linguists would draw, just as they also occasionally draw from a wider stock reaching beyond their own discipline.

But to think of it this way is not quite correct. A good theoretician knows one or more – preferably several – prior theories. But he knows them as wholes, more or less. The simile comes to mind of a second-hand parts dealer. He does not have an inventory of, say, crankshafts, all laid out on shelves, neatly classified, but a lot full of old cars. When the customer comes in for a crankshaft, he thinks over his stock, goes out and finds an appropriate old hulk, and retrieves a part from it. So a theoretician searches old theories for components he needs, and performance records of old theories to know something of what he can expect from them. And he raids old wrecks for what he requires. Every theory on the road today is, if we should examine it closely, a mélange of components, some going back to hoary antiquity.

That picture over-does one aspect. Not often is the search that wide. Much of the structure comes from an ambient basic set of theoretic convictions, some popular fads of the era, some old reliables. The latter constitute a sort of core theory, something upon which as a foundation new theories are elaborated.[18]

[18] The picture is incomplete in another respect. Many readily available components, instead of being scrounged, are from ignorance of their existence simply reinvented.[19] This is inefficient in two respects. The idea does not always arise spontaneously when required, so that there is often a long delay in filling a need, even when the required device has been developed. And certain information about the component amassed in previous trials is not available and must be laboriously worked out. Of course one can plead that invention and experimentation de novo is easier[20] than transfer from a different theoretic framework. But that can only be because too few theoreticians are adequately equipped for their tasks.

[19] In the Bloomfieldian period Northamerican linguists varied from reluctant to adamantly resistant to reading, e.g. Firthian papers.[21] The general attitude is persistent.

[20] And, incidentally, it is better received by Ph.D. committees!

[21] In their defense, it must be noted that the language barrier here is much greater than with, say, Prague or Geneva. – But they didn't read these either.

This core of theory has a close relationship with what we described a moment ago as the apparently ad hoc apparatus for stating and organizing the bank of results. There we used only certain of its properties. We needed only a meta-language and a comprehensive filing system. But closely tied to these, more or less inextricable from them, is a great deal more. We may consider the statement and organizing apparatus as only the surface of the mass, or by another metaphor, the tip of the iceberg.

If you will entertain the possibility that under this – and standing apart from what we usually call "theory" – there is a greater mass of concept and axiom, of meta-language and taxonomy, then we can ask: Where does it come from? What does it do? What part does it play in linguistics?

Consider a serious new theory – I mean merely some theory that is seriously proposed and debated in the profession. It makes its contribution, while it lasts, to the on-going discipline, *first,* by assisting in the turning up of new additions to the store of results, *and second,* by providing a proving ground for components of theory, some perhaps new, some only slightly tested. It is the background for development of new terminology; elements that are found useful will be added to the core meta-language. That is, every theory potentially contributes to this repository of theory and descriptive apparatus. Whether we like it or not, *EQUI-deletion* is for the foreseeable future a term in the general meta-language. What it stands for is a large body of "fact" now incorporated into the bank of "fact", whether we analyze it in that way or not.[22] And so with *phoneme,* neither the term nor the concept can be forgotten, even if you are fully convinced by Halle's argument that there is no such thing.[23] These terms have passed, from two quite different theories into that core of theory and meta-language which provides not only the apparatus to service the bank of results, but also the ground of theory, the foundation of theorizing.

We are dealing with much more than merely an ad hoc apparatus. This core theory must be placed alongside the bank of results as one of two central elements of stability and continuity in the discipline. They are alike in that their growth is continuous and cumulative, that they are largely immune to revolution, that they are common property of all parties, schools, and tendencies within the discipline.

7. *Theorizing and Heuristics*

The theories that come and go, the ones we debate about – and too often fight about – must have a more marginal status. Whatever our conceits in the matter, we cannot ask of any such theory whether it is true or false, anymore than we can ask whether it is green or blue, or parenchymatous or sclerenchymatous. These are all meaningless questions. We must ask, instead: Is it useful? Or rather: How useful is it? In what ways and for what purposes and under what conditions is it useful? What useful contribution can it make to the bank of results? to the core theory of the discipline? to applications?

These questions are different in some details from those we ask of heuristics: Is it useful? Under what circumstances? With what precautions? But there is a recurrent similarity through all of them. The key word is *useful.* Both are tools of the discipline, basically nothing more.

Theorizing and heuristics are more alike than we at first suggested. Yet the difference is actually real enough; it is just not one of absolute quality. It is rather one of scale, or level.[24]

[22] To insist that the analysis goes with the term is the etymologic fallacy.

[23] There is nothing wrong with a good ghost story. (And ghosts just *might* be real!)

[24] If this is so, then there might be more than two positions on the scale. There are some hints of some Bloomfieldians thinking in terms not only of *procedures* and *short cuts,* but also of a third apparatus to be used in polished statements. This latter would be farthest down the scale toward the theory pole, with *procedures* at an intermediate point. But, in any case, *procedure* remains in sharp contrast with *short cut,* and is certainly more nearly 'theory' than 'heuristics'. That is, the argument of section 1 could be greatly elaborated, but the general conclusion would not be changed: Bloomfieldian *procedures* are not "discovery procedures".

A specifc theory persists for a time, does its work, and passes from the scene, except as its elements are taken up by others. A heuristics is brought into a problem, does its work, and then is put aside until the next call for it. A specific theory might be thought of as a kind of heuristic, a sort of hyper-heuristic, one that serves on a much grander plane, but, like all such, useful only for its services to more central matters.

But making any such discrimination among the components of the discipline does not imply that some are important and some trivial. All four that I mentioned at the beginning – theory, heuristic, results, and applications – have their place. If, as I suggest, a further distinction is introduced between core theory and theorizing, making five, these five all have a symbiotic relationship, with each dependent on all of the others. The optimal health and well-being of linguistics – and its continuity as a productive discipline – depends on our recognizing this fact and giving each of them the attention and respect it deserves, and no more.

¡Madison Avenue, si, Pennsylvania Avenue, no!

James D. McCawley
University of Chicago

This paper deals with scientific revolutions and their relation to advertising, a subject that was touched on in the paper by Adam Makkai read at the 1974 LACUS Forum.

The notion of scientific revolution, popularized particularly by Thomas Kuhn's 1962 book, has figured in much discussion of the recent history of linguistics. Kuhn's ideas, however, have often been grossly misunderstood; for example, among those who have but a nodding acquaintance with Kuhn's work, there is a deplorably common tendency to form an unholy synthesis of Kuhn's notion of revolution with the previously standard view that science develops cumulatively, which yields the popular but totally unwarranted view that scientific revolutions are always for the better. I note in passing that Chomsky's conception of the history of linguistics commits him to the view that there have been scientific revolutions for the worse in linguistics and psychology (e.g. the 'neogrammarian revolution' and the 'behaviorist revolution'). Since the notion of scientific revolution and other notions with which Kuhn works have been subject to a wide variety of interpretations, many of which Kuhn has explicitly repudiated (1970, 1974), it would be worth my while to begin with a brief sketch of the conception of 'scientific revolution' which I will assume below. The reader is cautioned that what follows may not be exactly what Kuhn had in mind.

Kuhn 1962 devotes little explicit discussion to what is perhaps the most fundamental notion in his conception of the history of science, namely the notion of scientific community[1]. A scientific community is first of all a community: it is a body of persons which is potentially immortal, in that new members can enter it without its losing its identity, and such that its members communicate with each other (often via highly indirect paths of communication) and have some awareness of the identity of this body to which they belong and some concern for the future of the body and of their relationship to it. What distinguishes a scientific community from other kinds of community, such as a religion or an artistic clique, is that the activities relevant to the identity of the community have to do with the acquisition and refinement of knowledge. I emphasize that the <u>refinement</u> of knowledge is essential to the status of a community as a scientific community: the life of the community must include not only the accretion of new knowledge but the replacement of existing knowledge by better knowledge and the purging of old 'knowledge' that has been found to be in error. (Found by whom? Read on for an eventual answer.) According to this highly informal characterization, Catholic theologians might form a scientific community, but the Catholic

religion would not. I am not disturbed to have a community of theologians coming out to be a 'scientific community': the term ought to be broad enough to include freakish and nonproductive scientific communities as well as scientific communities such as we would be likely to approve of, since, following Kuhn, I want to separate clearly between the question of how scientific communities do in reality develop over the course of time and the question of what are valid grounds for accepting a scientific theory. My characterization allows for the possibility of a community becoming a scientific community, or of a scientific community losing the status of a scientific community though still remaining a community, or of a scientific community splitting into two or more separate communities, all or some or none of which constituted scientific communities. For example, I would hold that the community of historians has gradually been turning into a scientific community over the last century.

Kuhn 1962 used the term 'paradigm' in a bewildering range of ways, as has been noted by critics (Shapere 1964, Masterman 1970) and admitted by Kuhn himself (1970, 1974). The sense of 'paradigm' that will be most important in the discussion below is what a linguist might describe as a set of 'markedness principles'. At any time, in a scientific community, there are certain factual and theoretical claims and approaches to the solution of problems which have acquired such prestige and/or wide acceptance that members of the community feel free to employ them without offering further justification for them; these ideas and approaches will generally be what appear in textbooks and courses that are intended for elementary instruction in the subject that the scientific community is concerned with. These claims and approaches are 'unmarked', as contrasted with 'marked' claims and approaches, which students will be exposed to (if at all) only after they have a background in the unmarked approaches; a person presenting a paper at a meeting can not just assume 'marked' claims or approaches but must also defend them, or at least, he must if he is to retain his standing as a member of the community and not acquire the reputation of a gate-crasher or a crackpot. Note that a member of a scientific community can contradict claims of the community's paradigms and still remain a member in good standing of the community; however, he has to exert effort in order to do so, namely the effort of offering (rather than presupposing) justification. A paradigm need not even be consistent: several mutually inconsistent approaches may each have acheived sufficient status within a scientific community that any may be adopted without further ado.

Within any scientific community there are likely to be subcommunities which have different markedness principles than the whole community does. This will almost certainly be the case whenever the large community has an inconsistent paradigm, e.g. within the community of physicists, among which several conceptions of atomic structure are 'paradigmatic', there are subcommunities whose paradigms include only one conception of atomic structure. A similar situation prevails in linguistics; for example, in a presentation at an LSA meeting one could assume without further ado the interpretive-semantic version of transformational grammar presented in Jackendoff 1972, but one would be

required to offer some justification for it in presenting a paper in that framework at a meeting of the Chicago Linguistic Society. Note that thus a person can recognize a proposition as part of the paradigm of a community to which he belongs even if he does not accept that proposition himself.

How can a situation such as this come about or prevail more than briefly? Wouldn't a proposition of the paradigm have to have been refuted before a subcommunity that accepted a conflicting proposition came into being? Once a proposition of the paradigm has been refuted, won't all reasonable persons recognize that it has been refuted, and won't it thus lose its status immediately as part of the paradigm? And won't a new analysis that works where the existing paradigm has failed rapidly become accepted as a component of a new paradigm?

Far from it. The grounds on which it is appropriate for one to reject or to accept a proposition or a theory or an approach are nowhere near as clear-cut as they are often held to be. There can be reasonable disagreement among individuals as to what putative facts should count as facts and what facts should be regarded as having relevance to the given scientific discipline. For example, the followers of Velikovski accept ancient myths as providing reports (albeit, distorted reports) of actual cataclysms and take the agreements among myths of different cultures as facts that require explaining and which bear on geology and astronomy; but 'mainstream' geologists and astronomers do not feel obliged to account for these supposed facts. In this and many similar cases, there are no clearcut grounds for agreeing with one group against the other. There can also be reasonable disagreement as regards what features of a scientific theory are grounds for harboring doubts about it. For example, many first-rate physicists have viewed with great suspicion the 'action at a distance' which is central to Newton's theory of gravitation; indeed, Newton himself accepted it only with grave misgivings. But there are also many first-rate physicists who aren't bothered a whit by action at a distance. Further, as Feyerabend (1970) has argued, the fact that a theory has been refuted does not mean that it will stay refuted for all time. Supposed refutations of a theory generally rest on auxiliary theories and factual claims, some of which might well be wrong. Feyerabend notes that the heliocentric theory of the planets, which has been kicking around since antiquity, stood as refuted until around 1600, when a new theory of motion was developed that allowed the heliocentric theory to be reconciled with the facts. An adherent of a theory that has been refuted has several honorable alternatives available to him. If there is an alternative theory available that works where his theory fails, he may simply junk the one theory in favor of the other. Or he might attempt to construct a variant of his theory which accords with the facts, by pinpointing some aspect of that theory that can be held responsible for the discrepancy with the facts and revising that particular point of the theory so as to eliminate the discrepancy. Or one might identify a particular area that an as yet non-existent auxiliary theory might cover, and act under the assumption that an auxiliary theory will eventually be forthcoming which will bring his theory into line with the facts[2]. This last alter-

native amounts to issuing a promissory note on behalf of the theory, and it is not wise to do that unless the theory's credit rating is good, i.e. unless the theory's value as a tool of explanation and prediction has been sufficiently high as to warrant confidence in it. But of course members of a scientific community can be in reasonable disagreement as to the credit ratings of various theories. And one generally cannot be sure which of these alternative responses to a supposed refutation is the right one until long after the fact.

Thus, at any moment in its history, a scientific community may have a 'paradigm' and yet harbor considerable disagreements among its members not only about matters on the 'frontiers' of the science, but even about points of the paradigm itself, and these disagreements need not reflect any ignorance or moral turpitude among the parties to the disagreement. These observations set the stage for what Kuhn calls 'paradigm shift', which is the essence of a scientific revolution. Paradigms change in the course of a scientific community's history, sometimes in minor ways and sometimes drastically. In the case of a major shift, the change in the paradigm of the larger community begins with the development of a subcommunity which accepts the new paradigm (or some embryonic form of the new paradigm) and culminates when that subcommunity has swelled in numbers to such an extent that it is hard to distinguish between the subcommunity and the whole community.

The full story of scientific revolutions will then have to cover both how subcommunities come into being and how subcommunities can grow (or diminish) in importance within the larger community. This is where advertising starts to raise its head. Journals and professional meetings provide forums that are an essential part of the business of extending and refining knowledge. Since a paper that merely reiterates points of the paradigm will be a waste of time for all concerned, a large proportion of the papers presented in journals and at meetings will contain ideas which are not part of the paradigm, and which may perfectly well conflict with the paradigm. Meetings and journals facilitate the formation of subcommunities: they allow holders of non-paradigm positions to identify others of like mind, and they expose the readers and participants to presentations of ideas or viewpoints that they previously had not been aware of or had not attached much importance to, possibly influencing some members of the audience to accept those ideas or at least to take an option on them and invest some of their intellectual capital in exploring their consequences. Such presentations, whatever else they are, certainly constitute advertising. They present the audience with grounds for adopting the ideas in question, by illustrating what can be done with them and by pointing out advantages of those ideas over other ideas that might be harnessed to do the same work. When the new ideas conflict with a person's current ideas, selling him the new ideas will be difficult in proportion to the use that he gets out of his present ideas, since (Kuhn 1962: 76) 'As in manufacture, so in science -- retooling is an extravagance to be reserved for the occasion that demands it'. To put it on a more mundane level, if you want to sell your can openers to people that already have can openers, you aren't going to succeed just by telling them that your can openers open cans. You will have to con-

vince them that your can openers have advantages over their present can openers, and not just any advantages, but advantages that they care about or that you can make them care about. A person might junk his present can opener in favor of one of yours because yours makes it harder for him to cut his fingers, but not because it plays the Star-spangled Banner while you turn the handle. In summary, to win an adherent to a new idea, you must convince him that the expenditure of intellectual capital that is involved in his adopting the idea will pay off in things that matter to him.

Let's turn now to advertising in general. Makkai compares advertising with witchcraft and sees a need for exorcists to free the victims of advertising from its spells. He accuses transformational grammarians of recruiting adherents to their paradigm through the practise of this black art. By contrast, he says proudly of stratificational grammar, 'We do not advertise' (1974: 207).

This last claim is just as much nonsense as was Newton's claim that he didn't construct hypotheses. Of course, stratificational grammarians advertise for their theory, and Makkai's paper is a perfect example of advertising on the part of stratificational grammar. This doesn't mean that Makkai and Lamb are practising a black art; they are engaging in a perfectly ordinary activity that is practised by everyone from the pros on Madison Avenue to the pastor announcing next Sunday's sermon. In treating professional advertising as witchcraft, Makkai is grossly overestimating the power of advertising and grossly underestimating both the intelligence and the inertia of the audiences at whom advertising is directed[3]. As an illustration of this point, consider what effect the cigarette commercials that Makkai describes with great indignation have actually had. From 1961 to 1970, the per capita consumption of tobacco by adult U.S. residents decreased steadily, and the per capita consumption of cigarettes also decreased, though somewhat more erratically:

Year	Pounds of tobacco consumed per U.S. resident ≥ 18 years old	Number of cigarettes consumed per U.S. resident ≥ 18 years old
1925-29	9.68	1285
1930-34	8.80	1389
1935-39	9.22	1779
1940-44	10.88	2558
1945-49	12.46	3459
1950-54	12.61	3695
1955-59	11.71	3806
1960	11.82	4171
1961	12.00	4266
1962	11.80	4265
1963	11.78	4345
1964	11.54	4194
1965	11.51	4258
1966	11.12	4287
1967	10.70	4280
1968	10.59	4186
1969	10.04	3993
1970	9.68	3985

In the early 1970's, while per capita consumption of tobacco as a whole continued to drop, per capita consumption of cigarettes increased to a figure approaching that of the early 1960's:

Year	Pounds of tobacco consumed per capita (U.S. adult)	Number of cigarettes consumed per capita (U.S. adult)
1970	9.68	3985
1971	9.54	4042
1972	9.46	4080
1973		4100 (estimate)

(Source: Tobacco situation, Washington: U.S. Dept. of Agriculture, March 1971, p. 5, and Sept. 72, p. 6; New York Times Index 1973).

Jan. 3, 1971 is an important date in this story, since that was the last day on which cigarette commercials could legally be shown on American television. The above figures show that while television viewers were being exposed daily to these diabolical cigarette commercials, their average consumption of tobacco in general and cigarettes in particular was decreasing, but as soon as the commercials were taken off the air, they started smoking more cigarettes. These figures suggest two things to me. First, that cigarette commercials in the 1960's led very few people to start smoking and led very few smokers to break a resolution to stop smoking: rather, their principal effect was on what brands smokers smoked[4]. Their effect is thus something of no consequence to me. While I consider smoking to be about as polite as farting, I couldn't care less whether the noxious fumes that my neighbors are forcing me to breathe come from a Marlboro or from a Salem. Secondly, the figures suggest that the anti-smoking ads that viewers saw only once in a rare while were more effective at turning people off of smoking than the cigarette commercials that they were deluged with night after night were at turning people on to smoking.

I have a more general conjecture, namely that people are a damn sight more rational in their responses to advertising than they are often given credit for being. In particular, they are aware of the implications of Grice's (1975) 'conversational maxims', i.e. they draw conclusions not only from what the advertisement says but also from the fact that it doesn't say other things that it might have said. If someone is trying to sell you something, it is in his interest to give you reasons why you should buy it: 'If you've got it, flaunt it', as the National Airlines ads used to say. Using Grice's principles, one can infer that if an ad doesn't flaunt anything, it must because there isn't anything for it to flaunt. The cigarette ads that Makkai cited didn't flaunt anything, and the most that any of them was able to bring about was that a significant number of smokers happened to think of that brand when their regular brand was sold out or when they for some reason felt they would rather switch than fight.

I've been talking about advertising which is merely vacuous; let's now turn to advertising that is deceitful or irresponsible. I maintain that irresponsible advertising is far more common in amateur advertising than in professional advertising. To illustrate this point, let's look at something that isn't generally thought of as advertising but which obviously is if you just think about it, namely university course catalogs. Course catalogs constitute advertising fully as much as the Sears-Roebuck catalog does: they are distributed

to prospective customers and provide information about the seller's products that the prospective customers use in deciding what if anything they will buy from the seller[5]. Viewed as advertising, course catalogs are often disgracefully irresponsible. They are often prepared far in advance and based on guesswork as to what faculty members will be around 3 or 4 years hence and what courses they will be offering, but only rarely do they contain any indication of the tentative nature of their listings. If, as is often the case, many of the superstars on the university's faculty spend much of their time away from the university, that fact is not noted in the catalogs. A department's course listings often reflect the chairman's wishful thinking about courses that he'd like to see offered if he could get a new position budgeted and could find the right person to fill it. Catalogs often list obsolete degree requirements which no one on the faculty took the trouble to read and correct.

Universities are able to get away with irresponsible advertising simply because the people concerned, both purveyors and consumers, do not think of it as advertising and do not demand of it the standards that they would demand of advertising by a mail-order house or an insurance company; for example, students don't bring lawsuits against universities whose catalogs have led them to waste time and money on courses that were not what was advertised. I enter a cautionary note here: to say 'We do not advertise' is to court disaster; to keep from producing irresponsible advertising, one must make an effort to recognize his advertising as such and to impose on it the standards that he would impose on advertising in general. In fact, Makkai's paper contains many irresponsible claims and suggestions about the theories he is arguing against and how they differ from his theory, as when he cites as an advantage of stratificational grammar the fact that 'we do not make the mistake of deriving the participial interpretation [of Visiting relatives can be a nuisance] from the gerundival one, or vice versa' (p. 202). To my knowledge, no one has ever made that mistake, least of all his antagonists, who are unanimous in deriving the two interpretations from two distinct underlying structures[6]. Shades of the Lucky Strike ads that said 'It's toasted', when in fact all cigarette tobacco is toasted.

In this attack on Makkai, I have espoused propositions that seem to be leading me towards Makkai's conclusion that advertising in scientific communities may be particularly pernicious and may result in scientific revolutions for the worse: I've agreed that advertising plays an important role in scientific revolutions, and I've maintained that amateurs are more irresponsible advertisers than professionals are. Let's then see to what extent perversions of advertising can be expected to occur and to have pernicious effects in scientific communities.

Deceitful or irresponsible advertising can gull suckers into buying an inferior product; however, it is powerless to lead a sucker to buy the same product a second time A corollary to this observation is that deceitful or irresponsible advertising can be of more than transitory benefit to the advertiser only if the product is something that one doesn't buy very often. Even here, the incidence of irresponsible

advertising is kept fairly low by the fact that the bilked or injured customer is often in a position to bring a lawsuit against the offending company and win it, and it is in the company's interests to keep from losing lawsuits. Another corollary: irresponsible or deceitful advertising is potentially most effective in the case of products which a consumer buys only rarely and where his dissatisfaction with a particular product does not provide grounds for a lawsuit. It now looks as if Makkai is onto something: scientific theories are something that one doesn't buy very often (remember Kuhn's remark about avoiding the expense of retooling) and furthermore, no one could sue a scholar for having sold him a lousy theory, since the transactions in the market for ideas do not involve the transfer of money. The market for ideas thus seems to have all the characteristics of a market where irresponsible or deceitful advertisers could have a field day. And in fact, the market for ideas has a pretty bad record as far as markets go. It is much easier to find revolutions for the worse in the market for ideas than in the markets for more mundane things like writing instruments; there is no instance that I know of in which one type of writing instrument has supplanted another in which the new writing instrument did not enable people to do more writing, do it faster, and do it more cheaply.

I think that suckers do get fleeced worse in the market for ideas than in the market for writing instruments. However, most of the wrath that this idea stimulates in me is directed not against the swindlers but against the suckers. A scientific community gets the scientific revolutions that it deserves. The members of a scientific community are in the business of acquiring and refining knowledge, and they thus ought to be adept at evaluating claims. If the community is filled with persons who have little awareness of the implications of their supposed knowledge and little concern with the critical evaluation of other ideas, people who are willing to invest their intellectual capital on a scientific vehicle that they haven't given a thorough test-drive, then the community is in bad shape: it is in serious danger of mass investment of its collective intellectual capital on worthless projects. However, if a community is in that bad shape, its members can't have all that much intellectual capital to be bilked out of. If anything, a scientific revolution, regardless of the relative merits of the new and old paradigms, is likely to do good for that kind of scientific community, since any kind of scientific revolution would be likely to stimulate some members of the community to the critical thinking that the community has been lacking in. In the case of a healthy scientific community, irresponsible advertising is likely to be ineffective, since the community will be full of people adept at detecting its irresponsibility. Indeed, the only real danger of irresponsible advertising in a healthy scientific community is that if a good theory is irresponsibly advertised, it will become a laughing-stock and not be given as serious an examination as it deserves. Thus, my impression of advertising in science is that it is the lifeblood of scientific communities, and that even irresponsible advertising does as much good as it does harm.

So far, I have said nothing about the other thoroughfare that is mentioned in my title, Pennsylvania Avenue. I will conclude by recti-

fying that omission. When a subcommunity develops within an existing scientific community, it is impossible to determine whether that subcommunity forms the vanguard of a scientific revolution. In its early stages, a subcommunity will have only a fragmentary paradigm, and its members will have extended more credit to that paradigm than its credit rating really warrants; their membership in the subcommunity will to a certain extent represent a speculative investment of their intellectual capital[7]. The subcommunity's paradigm may become more highly articulated and may provide solutions to important problems, and the auxiliary hypotheses needed to pay its debts may be developed, so that its credit rating improves and it attracts investment of a less speculative character, possibly culminating in its paradigm becoming the paradigm of the larger scientific community. On the other hand, the subcommunity's paradigm may just go deeper into intellectual debt without ever developing beyond mere fragments, and it may lose all power to attract additional investment. If the investors are rational and well-informed (they of course won't all be), a subcommunity's paradigm will become that of the larger community only if it has shown itself over a period of several years to be a worthwhile investment. Now let's see how government spending on research and education alters the picture. I maintain that government subsidization of research and education, regardless of how benevolently and fairly it is administered, increases the likelihood of scientific revolutions for the worse, since it makes it possible for a subcommunity to increase its membership drastically without demonstrating that its intellectual credit so warrants. The kind of development that I have in mind is illustrated by the rapid growth of American universities during the late 1950's and the 1960's, stimulated by massive spending by the federal government. This spending made it possible for many universities to start linguistics programs that otherwise would not have been started or would not have been started so early, or to expand existing programs much further than they would otherwise have been expanded. Given the situation of the early 1960's, it was inevitable that a large proportion of the new teaching jobs in linguistics would go to transformational grammarians. In the case of new programs, since at that time transformational grammar was the kind of linguistics in which it was most obvious that new and interesting things were going on, many administrators would prefer to get a transformational grammarian to organize the new program; in the case of expansion of existing programs, even when those who had charge of the new funds would not speculate their personal intellectual capital on the new theory, it was to their advantage to speculate their newfound monetary capital on it, since if the new theory was going to become influential, a department would have to offer instruction in it if the department was to attract students in numbers that were in keeping with its newfound monetary riches. And with the first couple of bunches of students turned out by the holders of these new jobs, the membership of the transformational subcommunity swelled greatly.

In saying this, I am not saying that it is a bad thing that the transformational paradigm acquired its position of eminence in linguistics. What I am saying is that it acquired that position faster than it deserved to and that the mechanism which speeded up its

acheivement of that position could equally well have done the same for a paradigm that was headed for intellectual bankruptcy but which, at the time when the manna was raining down from heaven, looked like the wisest investment[8].

FOOTNOTES

1 Kuhn makes up somewhat for this omission in Kuhn 1970, 1974.

2 This alternative is not radically different from the 'puzzle-solving' that Kuhn takes to be a major part of 'normal science'. Putnam (1974) describes 'puzzle solving' as finding the auxiliary hypotheses which, in conjunction with the assumed theory, will imply given facts, that is, 'solving the syllogism for A':

Theory T (given)
Auxiliary hypotheses A (to be found)
Facts F (given)

A promissory note is issued when the community does not quickly come up with a solution to the syllogism.

3 For many valuable comments on the lack of power of advertising to engage people's attention, let alone to sell them something, see Cone 1973.

4 Things were different in the 1920's, when many cigarette advertisements were concerned with popularizing smoking in a previously untapped market, namely women. The ads popularized both the idea that women had just as much right to smoke as did men and the idea that smoking could serve a useful purpose ('Reach for a Lucky instead of for a sweet'). Given that in the 1920's the connection between sweets and fatness was more widely recognized than the connection between smoking and cancer, these ads were not at all objectionable: they helped wipe out a stupid reason for not smoking and helped spread something that, relative to the time, was a good reason (albeit not an overwhelming one) for smoking.

5 It is unfortunate that people in academia, whether students, faculty, or administration, rarely think of a university's students as its customers. Greater awareness of the buyer/seller relationship would be advantageous to, if not all, at least most of those concerned. Students at present do not give enough thought to the question of how best to get full value for their (or their benefactors') money, and faculty members think very little about the question of whether their students are getting their money's worth. Much student unrest could be avoided by judicious application of the principle that the customer is always right, i.e. by recognizing that the university in accepting the student's money is entering into a contract with him and the student has the right to decide for himself whether he is getting what he paid for and to cancel the contract and get a refund if he isn't.

6 Here are a couple of more misrepresentations from Makkai's paper. (1) Makkai indicates (top of p.201) that transformational grammarians subscribe to the proportion 'Surface structure is to deep structure as

performance is to competence'. However, the notions of competence and performance are applicable with reference to any level of linguistic structure. (It is especially clear that 'competence' has to do with all levels, since a grammar is supposed to be a formal account of competence, and a grammar includes rules relating to all linguistic levels). (2) Makkai applies the term 'publicly verifiable' to Lamb's 'complexity count' in such a way as to suggest that he is citing a difference between stratificational grammar and transformational grammar. There is in fact no dearth of 'publicly verifiable' methods of measuring 'complexity' of the grammars of a given theory. What is lacking is a 'publicly verifiable' basis for choosing among the possible 'evaluation measures' or for deciding whether the notion of 'evaluation measure'/'complexity count' is even of any relevance to a linguist's concerns (see McCawley 1975 for arguments that it isn't). Makkai's aversion to first-order arbitrariness is coupled with a willingness to accept second-order arbitrariness, much in the same way that Immanuel Kant (Kaufmann 1973: 239-41) lived the minutest details of his life according to universal maxims but was quite arbitrary in his choice of maxims to live his life by (for example, he adopted the maxim that one must not smoke more than one pipe of tobacco per day).

7 There is nothing to prevent the investor from spreading his intellectual capital among several speculative ventures. The more speculative a venture is, the wiser it is not to risk everything on it.

8 At this point, one might ask whether I should be condemning just government subsidies for education and research, rather than subsidies from any source, public or private. I in fact maintain that private support for education and research is far less likely to cause undesirable or premature scientific revolutions, since the mechanics of private subsidy produces a tendency towards diversity, whereas the mechanics of government subsidy produces a tendency towards uniformity. When a government subsidy program goes into effect, all universities have potential access to the funds, the same kinds of uses are made of the funds in different places, and the same ideas will influence the recipients of the funds with regard to how they should spend them. By contrast, private subsidies are more sporadic as regards who gets them and when they are available, are distributed and used in a great variety of ways, and the individual donor, even if he is H. L. Hunt, has nothing near the funds to pour into a single project that the federal government does.

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THE ETHNOGRAPHY OF LINGUISTIC INTUITIONS AT WARM SPRINGS[1]

Virginia D. Hymes
University of Pennsylvania

Warm Springs is the same American Indian community whose rules of speaking Susan Philips discussed at the 1970 Georgetown Roundtable. She was concerned with their implications for the interaction of Indian children with non-Indian teachers in local classrooms.[2] I will be concerned with their implications for the interaction of linguist and native speakers in the collection and interpretation of linguistic data.

Interaction between a linguist and speakers of the language he is studying is a speech situation. Just as in all speech situations, there is an interplay between cultural and more narrowly grammatical rules in determining what is said, and what is understood by it. In this paper I concentrate on the relevance of this fact for my work at Warm Springs, but ask you to consider its wider relevance.

The data which result from the interaction between linguist and native speakers, and among native speakers themselves (as, for example, in natural conversation) can be looked at as belonging to three categories: production of utterances, understanding of utterances, and judgments about utterances in the language.[3] Linguistic work requires all three kinds of data, and sociolinguistic rules affect all three. If the linguist is not aware of the rules of speaking operating in the situation in which the data were produced, he is in a poor position to evaluate any of the three.

There are a variety of ways of obtaining linguistic data. Linguists obtain it by consulting written texts, consulting their own intuitions about their own language, consulting other speakers of their own language (as in interviewing), consulting speakers of a language they don't know (elicitation) and by collecting natural conversation with or without participant observation (snooping?). Some of these lead to recognition of sociolinguistic rules much more quickly than others.

Linguists working on American Indian languages, a tradition of work that has played a vital part in the development of descriptive linguistics in America, have obtained data in most of these ways. The notable exception—or near exception—is consultation of one's intuitions about one's own language; there have been only a very few linguists who were native, or even fluent, speakers of the American Indian languages they worked on.[4] Let me now give a characterization, almost a caricature, of one way linguists have worked on American Indian languages. The linguist using this method of obtaining data, either through necessity or preference, went into a community and sought out a native speaker with the time, interest and, if possible, the special ability to serve as an informant. The work was done in almost total isolation from the community in which the language was spoken (if, in fact, it was spoken by others than the informant). Other speakers might be used either to make best use of the linguist's time or to check doubtful points. The data collected in this way was never intended to give a picture of the range of variation in the community. The important point here, however, is that this method only rarely, and then accidentally, gave the linguist insight into the rules of speaking of the community in which the language was spoken. Yet even in the one-to-one situation of linguist and informant those rules were operating and they affected the data and its collection.

It would be wrong to give the impression that all linguists working on American Indian languages have worked in this way or that it is impossible to do good linguistic work with a single informant. Sometimes both now and in the past there has simply been

no alternative. Often there were only one or two speakers of a language left alive and the linguist played a crucial role in preserving those last speakers' knowledge of the language both for scholarship and, as we now increasingly realize, for the descendants of those speakers. In the hands of its best practitioners the method of working intensively with one or two informants has resulted in valuable analyses of languages. Furthermore, there have always been linguists who by preference, or because their linguistic work was carried on in the context of ethnographic research, have combined participation in the life of the community with work with individual informants. While they were in a better position to gain insight into socio-linguistic factors affecting their data, they were still working largely for their own purposes and the results of their work were not intended for nor of much interest to the communities they worked in.

That linguists in the past could work on the languages of American Indian communities in isolation from those communities, for their own scholarly purposes, and without making the results of their work accessible[5] to those communities, was a reflection of a very different kind of situation on American Indian reservations than prevails today. Today American Indians have become actively involved in preserving their cultures and their languages. Having been used for so long as objects of study by scholars, including linguists, they are aware of the kinds of work scholars can do. Recognizing how valuable they have been to scholars, they now want to turn the tables and make scholarship work for them. They want this in two ways: they want scholars to work for them on projects designated by them, and they want scholars to teach their skills to tribal members. In linguistics there has already been progress in both directions. Many linguists now work for American Indian communities and some have trained or begun to train American Indians as linguists. What the new situation means is that it is less and less possible for linguists to do isolated work with a few informants to produce data not intended for the use of the community. On the other hand, because the American Indian community wants the linguist to work for it, for purposes that matter to it, the linguist is given the opportunity to enter into a working relationship with a wide variety of members of the community. He can take part in community meetings, teach classes, and often can carry on his linguistic work itself with a group of speakers of the language chosen by the community. He is thus in a situation in which, in order to interact effectively, he must learn the rules of speaking of the community. This is hard on the linguist who may have gone into our discipline in the first place because he wanted to be alone with a language. It is also hard on a linguist who wants to study only some one aspect of language, or use only some one method of obtaining data. If, as will typically be the case, the linguist's work is to produce materials which are to be used in the community, he must attend not only to what is said but also to what is the norm, because the materials must in many cases reflect that norm. They must also be based on sources which are recognized as valid sources of linguistic knowledge by the community, and in the collection of data and preparation of materials sensitivity to the community's wishes is essential. Though it can be frustrating and often exhausting to work on other people's terms, the new situation is not only ethically more attractive (they are *their* languages after all--not ours), but, as I shall try to show in what I say about my work at Warm Springs, this new situation can result in better understanding of the data one collects. Because, whether we intend this to be the case or not, all linguistic data, no matter how collected, is affected by the rules of speaking and attitudes about language of the community in which it is collected. The new conditions of work on American Indian languages can lead to quicker insight into these rules and attitudes.

In my own work on the Warm Springs variety of what linguists have called the Sahaptin language at Warm Springs reservation in Central Oregon, I have worked from the beginning within this modern framework. By the time I began work there in the fall of 1972 I would not have wanted to work in the old way nor would the community have permitted it. As one very outspoken acquaintance there put it: "If we thought you want-

ed to learn this language just for your own use, to just take it away with you, we'd run you off the reservation!" I was at the time working with a woman, active in teaching about her culture both on and off the reservation, who had wanted help in assembling a brief dictionary of her language. Something of the scale she had in mind had already been done by a linguist, based on work at Yakima. The rejection of this work by her and by others on the reservation reflected an attitude which must be taken into account in any work on the language at Warm Springs. The language as spoken at Yakima, at Umatilla and at Warm Springs has resulted from the splitting up onto three separate reservations of what was once a continuous chain of small local groups speaking related dialects of one language. One hundred years of reservation life has led to each community's calling the whole range of dialects spoken on its reservation by the name of that reservation and thinking of it as a separate language. Though there is widespread recognition of the fact of mutual intelligibility, the Warm Springs community does not think of materials prepared at Yakima as suitable for use at Warm Springs. For this reason, the woman with whom I was working had devised an orthography largely an adaptation of English spelling but with some features of the Yakima orthography and assembled a short word list or dictionary. She was not satisfied with her orthography, however, and so agreed to teach me Sahaptin in exchange for my help in working out a writing system. She made our working relationship widely known and took me with her to many community meetings and other events on the reservation. Since our work together was never a regularly scheduled, paid, separate part of her life, but rather was imbedded in the rather hectic pace of her many activities, I began learning many of the rules of speaking which I was to see in operation in my later work. Through her, and through my work with one other woman on a somewhat similar basis, I became known on the reservation as a linguist—as someone who could write and teach others to write the language. Thus by April of 1973, when the people involved in a new federally-funded program of teaching the Sahaptin language in the reservation school came to realize that they needed an agreed upon way of writing the language, it seemed natural to turn to me. Ideally there should have been a community member who was trained in linguistics, but as in most American Indian communities there was none and the immediate need could not wait on the training of one.[6]

It was arranged that I would work with the three teachers of the language and their supervisor, all native speakers of Sahaptin. We worked at first one and later two full days a week for five months. The three teachers were women—aged eighty, seventy and mid-thirties. Their supervisor was a man in his late sixties. He had worked during the nineteen fifties with an anthropologist from Reed College in Portland and with several students from there. He was familiar to some extent with what linguists do. Recently he had been teaching courses in reservation history and culture at the local high school and a community college about fifty miles away. Neither he nor the women were "educated" in the sense of having college degrees. Their education was something that was part of their lives in the community and they had been chosen because it was recognized that no amount of formal education could make up for the expertise they had in knowledge of the language and culture. The choice of the older people, at an age when the larger society would have forceably retired all of them, reflected the socio-linguistic fact that at Warm Springs (and probably many other American Indian communities) being a good speaker of the language is a matter of age and of involvement in the ceremonial and religious life of the community. Younger people may be fluent native speakers but the "real speakers" of the language are the old people. Over and over again I was told that I was lucky to be working with those two older women because they really knew the language. They were honored and respected elders. How closely this respect as elders is tied to being thought of as good speakers of Sahaptin was perhaps indicated by an incident at one community meeting. They and one other older woman (all three of them very close friends) were present as

representatives of "the old people". The meeting was to discuss what kind of materials should be developed for use in the schools to teach the culture to the children. Towards the end of the meeting, though all three women know and speak English, the whole evening's discussion was reviewed for them in beautiful, formal Sahaptin by the man who later became their supervisor in the teaching job. Their position as the embodiment of "speakers of the language" was thus highlighted by translating for them a discussion they had undoubtedly been able to follow in English. One might ask then how it was that *any* younger person was chosen to teach the language. It was undoubtedly a recognition of the fact that she, because she was a high school graduate, extremely articulate in English, and very reliable in her work habits would be a good teacher for the older children who were to learn to write as well as speak. It was a compromise between the demands of a modern school and the cultural expectation that children learn from their grandparents or other old people. There is no question, however, that the compromise imposed severe strains on the younger woman; she had to work extra hard to gain the approval she would have had automatically if she had been a great deal older.

My work with these four people took place in an unused classroom of the old BIA boarding school, and from the beginning (as in my work with the previous two women) all our sessions together were taped. It was through these early working sessions and through a series of night classes in writing the language that I gave during the summer months, that I became aware of how strongly the rules of speaking of the Warm Springs speech community affected my data—both its collection and interpretation.

I had noticed that the younger woman, though she played the role of intermediary between the older women and me, very rarely offered any forms herself unless they were temporarily out of the room or engaged with conversation with each other. This caused me to wonder if she was really secure in her knowledge of the language. The fact that she learned so quickly how the orthography we were developing worked and was able to be of help on doubtful points of spelling made it clear that she controlled the phonology. Perhaps she just didn't have full productive capability. It was not until, in one of my classes, I had made a mistake in word division in an example I had written on the blackboard that I got insight into what was really going on. No one mentioned the mistake but next morning when we were alone the younger woman told me about it. I said, "Why didn't you tell me? I wouldn't have minded. People know I'm only learning the language." Her answer was: "I couldn't say anything. Those older women would have thought I was showing off." I remembered then that there had been several older women present. She thus brought home to me a fact that was borne out by my previous knowledge of the reservation; at Warm Springs a younger person does not speak up if older persons are present who might do so. So strong is this rule that its operation in our work sessions had even led me to doubt this young woman's knowledge of the language she had spoken as her first language and in which she was perfectly fluent. Once aware of this rule I observed its operation all during the months of our work together and during the language classes I taught in the summer of 1974. In these classes, when she was the oldest person present, this same woman spoke up with authority and clear insight into the grammar of her language. Yet when older people were present she would remain silent most of the time, allowing them to make any comments or answer any questions. In general the oldest speaker present would serve as the authority, though several of more or less equal age and status in the community might share the role. Since this younger woman had worked with me for so long in the previous summer's group she started out the summer classes aware of the kinds of things I was looking for when I elicited forms. Yet even when someone offered forms which didn't exactly fit the patterns we were working on (as for example sentences illustrating person marking, or tense-formation patterns) she never offered any direct comments on or corrections of these inappropriate forms. Nor did anyone else, even older speakers.

This leads directly into another rule of speaking, or norm of interaction, at Warm

Springs which one had to understand in order to evaluate what went on in work sessions, and the data resulting from them. People at Warm Springs do not lightly criticize each other directly, especially in public. The way criticism or disagreement is managed is to offer a counterstatement so much later that it no longer seems directed at an individual, and to couch it in terms which also disconnect it from the person being criticized. Another device is to make the criticism to third parties and rely on the gossip chain to get it back to the person at whom it is directed.[7] Thus a linguist working with a group cannot assume that comments or corrections made by someone later are "just gossip" and therefore not to be relied upon. They may very likely be correct, or alternate, forms and must at least be taken as possibly valid sources of data. In my work I've had the experience of seeing someone appear to accept a form offered by another person, put that form on a tape that was to be used for teaching purposes, then at lunch time tell me that it had been a "fancy" form and not the usual word at all (which I'd known but been unable to say because I too was following this rule of speaking, as well as the one that requires deference to someone older), and suggest that we'd better hurry back and correct the tape before the other person got back from lunch! We did.

Another norm of interaction at Warm Springs which affects collection of linguistic data has to do with questions. Unlike our norm of interaction, that at Warm Springs does not require that a question by one person be followed immediately by an answer or a promise of an answer from the addressee. It *may* be followed by an answer but may also be followed by silence or by an utterance that bears no relationship to the question. Then the answer to the question may follow as long as five or ten minutes later. When this happens to you for the first few times, before you've learned the rule, you think you've asked a tactless question, so tactless that it has rendered the other person speechless or sent him into hostile silence, and you may hurry on to change the subject. The fact is that at Warm Springs there just seems to be no rule that a question demands immediate response. In fact, answering without time for sufficient thought is explicitly considered stupid, or heedless. The implication of this fact for the teaching situation is spelled out in the articles by Susan Philips cited earlier. What it implies for the linguist is that he must be aware of the rule or risk losing data. He must learn to wait in what is at first uncomfortable silence, and not *assume* that he has in any way embarrassed the person of whom he's asked the question. If the answer doesn't come he must learn to look for it later, often without any bracketing, and be able to refer it back to its question. (This is one way taping work sessions helps.) Furthermore, in his role in meetings in the community, or in classes he may teach, he has to learn also to expect that comments on what he or anyone else says may not directly follow, but may come much later. It is not unusual for a statement made at the beginning of a public meeting to pass without comment from anyone, for long discussion of other topics to intervene, and then just before the meeting is to end to have someone stand up and begin his comment on the opening statement. This may set off a long string of comments on the same statement. To rush on without allowing time for people to decide to stand and make their comments, or failing to realize that statements made at the end of a long meeting may be comments on statements made in the beginning, or answers to questions asked earlier on, can cripple one's effectiveness; people will go away feeling the meeting was a fake, that you didn't want to hear what they had to say; or, if you are a teacher in a class, that you are ignoring them. For the linguist, as for anyone else who wants to interact effectively with the people at Warm Springs, it is necessary to learn the rhythms of interaction that are natural in the community and not impose one's own. This is of course the case in all inter-cultural interaction.

When you work on a language of which you are not a speaker, you are, no matter what your theoretical position, dependent on native speakers' production of utterances, on their understanding of utterances (as for example when you must have their assistance in transcribing and translating conversational and other recorded texts) and on their evaluation of utterances. At Warm Springs I found that all the rules of speaking and

attitudes toward language which I have mentioned affected what community members I worked with did in regard to all three kinds of use of their knowledge of the language.

As mentioned, at Warm Springs being defined as a good speaker is not something that is objectively measured by fluency in the language. This affects the self-evaluation of younger speakers. They do not think of themselves as good speakers, or even as speakers, though they may be fluent. Even some who knew no other language than W. S. Sahaptin when they entered school in the mid-fifties are not mentioned by older people as speakers and do not, if asked, say they can speak the language. Yet having spoken only it until age six or so, and having been in an environment where it is often spoken, they are unlikely really to have ceased entirely to be speakers. Nevertheless their negative evaluation of their own speaking ability, combined with rules of deference to elders, make it difficult if not impossible to collect linguistic data from them. Since the speech of these younger people would provide evidence of trends of change in the language the difficulty of getting samples of their speech is an obstacle to obtaining a full picture of the present state of the language. It would be of great value to the community to have a linguistic census; it might reveal that there were more speakers than is commonly supposed and thus perhaps dispel some of the fear that the language will die. Yet the situation I have outlined would make such a census difficult to make; neither self report or others' report of speaking ability would be reliable. The cultural definition of who is a good speaker also affects any data which depend on one speaker's judgments about utterances of another speaker. The first question people always asked if I offered a form I had written down or had remembered was "Who told you that?". When they didn't ask it was usually clear that they thought they knew, and, in either case, how they evaluated the form depended on their evaluation of the source: whether the person was one who had the status of good speaker, whether they had personal knowledge of his speaking habits, what dialect they thought of him as speaking. This last point involves the community's conception of the language they speak as separate from those spoken at Yakima and Umatilla.

Forms which are different from the norm, though they may be common in the speaker's own usage, are most frequently labelled as being Yakima. This can affect even the possibility of using speakers' judgments of whether two forms which sound different are in fact the same or different words. For example: /l/ and /n/ are separate phonemes in Warm Springs Sahaptin. There is, however, in certain words an alternation between [l] and [n] which seems to be quite free. Yet for all these words the form with [n] is the one that people will say they use. The [l] forms when brought to their attention (as writing tends to do) are either rejected as Yakima or are explained as being used for some special meaning. The special meanings assigned to the [l] form of any one word vary widely from speaker to speaker and observation will show for any speaker I have encountered that the distinctions are in fact not maintained. Thus for example a person made aware of the fact that people say both *katnam* and *katlam* might claim that he used the first to mean long and the second to mean long-house. The next use of long-house, in a sentence elicited only a few minutes later, might show the [n] form. A clear example of how important it is to have the opportunity to observe natural conversation occurred in relation to this alternation between [n] and [l]. The group had rejected *miyalas* as a word for baby, saying it was a Yakima form, and that they as Warm Springs speakers said *miyanas* for both baby and child. (The *miyalas* form for baby had occurred on a tape made by Morris Swadesh at Warm Springs in 1955, from which I first began to learn Sahaptin before going onto the reservation). Since in myths the substitution of [l] for [n] is a common stylistic device, well recognized by people at Warm Springs as giving the effect of 'talking little', the alternation in these two meanings seemed reasonable. Sometime after the rejection of the [l] form, but on the same day, a young woman came into the room with a baby on a cradle-board. It was a new baby and everyone crowded around saying *miyalas* and *miyanas*, rather freely mixed in the kind of talking people always do to, or about, a new baby. Of course this is just one more example of the unreliability of

people's reports of their own speech or that of others. This unreliability is probably a true universal and there is no reason to expect the people at Warm Springs to be exempt from it. What is important is the need for the linguist to be aware of what attitudes (in this case an attitude to another reservation's language) are involved in the particular form this unreliability will take. Exposure to the same attitude in a wide variety of community members does make one aware of it.

In conclusion, working as I did at Warm Springs made it easier for me to learn the rules of speaking and attitudes about language which were affecting my collection of data and which were crucial to my interpretation of it. The need to produce materials acceptable to the community necessitated attention to norms as well as to what was actually said. It also limited to some extent possible sources of data. If, instead of being an outsider, I had been a community member who had spoken the language all his life and who had been trained as a linguist, I would still not have entirely escaped the effects of these rules and attitudes. In fact it might even have added new problems. If such a linguist were young, as any Native American linguist is likely to be, this would affect people's estimation of him as someone fit to work on the language. In addition, he himself would be operating on the basis of all the rules of speaking and attitudes toward the language sketched above, without perhaps having the consciousness of them that an outsider gets from their very jar with his own rules and attitudes. This is *not* an argument against training Native Americans to be linguists and to work on their own and other languages. (To help train such linguists is in fact probably one of the most important things linguistics can do in what remains of this century.) It *is* to argue that for a Native American linguist working on his own language, or in fact any linguist working on his own language, all the sociolinguistic facts which can affect the quality and interpretation of his data must be brought to the conscious level—as part of his training as a linguist. There is no direct access to linguistic intuitions; they are always mediated by cultural factors.

FOOTNOTES

[1] My work at Warm Springs was supported partly by a Danforth Foundation Graduate Fellowship for Women. Work with the teachers and classes taught during the summer of 1974 were supported by a grant to the Confederated Tribes of Warm Springs through Central Oregon Community College.

[2] cf. Philips, 1970, 1972 and 1974.

[3] T. G. Bever and D. T. Langendoen suggest this three-way division of a speaker's knowledge of his language in their article, 1972 p.33.

[4] For a discussion of the need to remedy this situation see Kenneth Hale, 1972.

[5] Some linguists in the past actually did make copies of their work on a language available to some members of the community. However, since they were not written to be read by the community they were still largely inaccessible. The more common pattern now is for a linguist working with a group on its language to produce materials intended to be used by that group. The ultimate result of my work at Warm Springs is to be a pedagogic grammar of the language and a comprehensive dictionary, both usable by the people there as well as other linguists. A long word and phrase list and tapes and Language-Master cards to accompany it have already been produced and are distributed by the tribe.

[6] One goal, however, of the classes taught in 1974 was to acquaint people with the way a linguist approaches a language. Ultimately the hope is that some few people will become seriously interested in becoming trained linguists. As Hale (1972, op. cit.) points out we must not assume that this means going away to a University to work through a regular B.A., M.A. and Ph.D. program in linguistics—though some Native Americans may want to do just that especially if a suitable university is within easy enough reach of their reservation to allow them to maintain their ties there. Linguistics must take seriously its obligation to provide alternative ways for Native Americans to receive training as linguists.

[7] Suggestions to another person to do something are handled with similar indirection. It may take an outsider a while to discover that a simple comment "They're showing some nice slides in the meeting room" slipped into a conversation around a campfire is actually a suggestion to go watch those slides so the speaker can carry on a private conversation with a third party. Or that a statement at a meeting "If anyone has anything to say we'll be happy to hear from them" is actually an invitation to the outsider to speak. Failure to do so may occasion the comment later about how happy they were to have you there even if you didn't speak up—perhaps some other time? Seeing this indirection in operation in giving suggestions to others as to what they should do during the preparation by the women of elaborate feasts for hundreds of people with amazing speed and efficiency is a source of amazement to outsiders. No-one seems to be telling anyone else what to do, yet it all runs smoothly. This is so partly because everyone knows what needs to be done, but also because these indirect suggestions are constantly being put forward and acted upon.

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AN EMPIRICAL CONSTRAINT ON THE CLASS OF POSSIBLE TRANSFORMATIONAL GRAMMARS OF NATURAL LANGUAGES

Robert Rodman

University of North Carolina

Theoretical linguists of all persuasions face the task of constructing a theory of language that is at once strong enough to specify a class of grammars that encompasses all possible human languages, and weak enough to give non-trivial status to the notion "possible grammar of a human language." Transformational-generative grammarians are not exempt from this general requirement. It is well known that the various transformational theories of language developed in the past twenty years all suffer the same defect: they are not sufficiently weak. They are not constrainted enough to define a class of grammars so narrow as to prevent grammars from being specified that could not possibly be natural language grammars.

In this paper I shall investigate the empirical consequences of adopting a meta-theoretical principle that narrows the class of grammars specified by transformational theory. The investigation will concentrate solely on the ramifications that the principle might have on possible grammars of English. I hope to show that the effect of the principle will force us to accept (or reject) grammatical analyses of fragments of English that we find to be superior (or inferior) to other analyses on independent grounds.

Any transformational-generative theory of language specifies that grammars provide for sentences at least two significant levels of representation: an <u>abstract level</u> and a <u>concrete level</u>. The concrete level most faithfully represents the linguistic structure of actual utterances; the abstract level is conceived in such a way as to facilitate the representation of the meaning of utterances. In some theories a third level called <u>deep structure</u> is defined whose purpose is to capture syntactic but not necessarily semantic generalizations. In theories where deep structure is not an autonomous level, we can view it as coinciding with the level I have called "abstract." These three levels are pivotal among the various formal devices of the theory. For example the notion "a sentence is two-ways ambiguous" is captured by assigning to the sentence one structure at the concrete level, one or two deep structures, and two structures at the abstract level.

The power of transformational-generative grammars to assign multiple representation on a particular level to a particular sentence can be empirically constrained. For example, a sentence that is two-ways ambiguous <u>must</u> be assigned exactly two distinct abstract structures. The effect of an empirically governed principle such as this is to restrict the class of grammars defined by the theory, hopefully without sacraficing the grammar of some natural language. Constraining the theory in this way is one of the

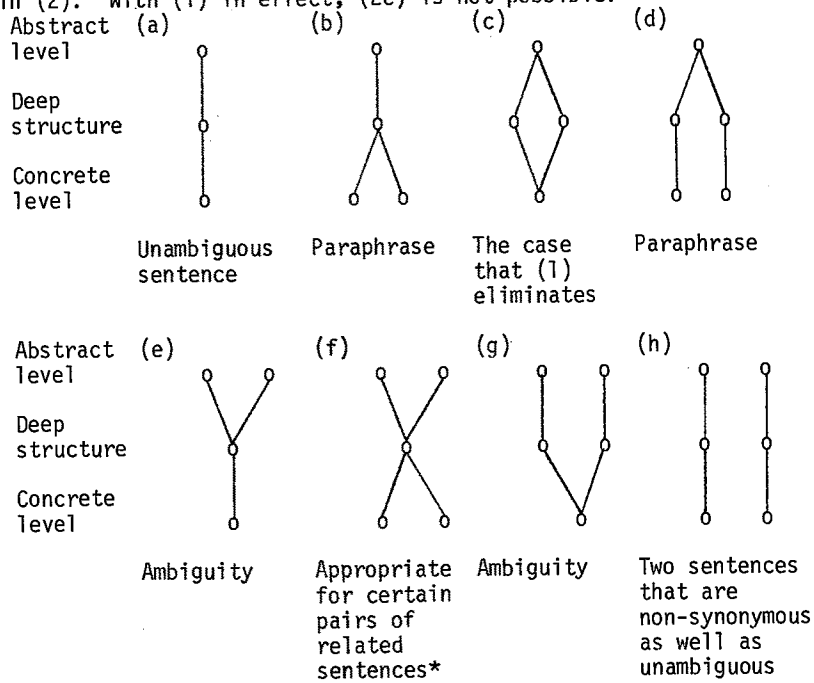
goals of transformational grammarians, and in furtherance of this goal I wish to consider the following meta-theoretical principle:

(1) Transformational theory admits no grammar that assigns to a sentence a single concrete structure, N deep structures, and less than N abstract structures.

In other words a sentence cannot have more deep structures than abstract structures assigned to it. As a specific example, if a sentence is unambiguous it cannot have more than one deep structure.

To put this constraint into perspective let us consider all the possible ways of assigning levels of representation to sentences. For brevity we confine ourselves to just two-way ambiguity and paraphrase. Without (1), there are eight possible configurations as shown in (2). With (1) in effect, (2c) is not possible.

(2)



*For speakers who find unambiguous both Everybody here speaks two languages and Two languages are spoken by everybody here, (2f) may be appropriate.

Eliminating grammars that allow (2c) not only constrains, but simplifies transformational theory. In theories where deep structures are very similar or identical to abstract structures (e.g., Generative Semantics), the need for meaning postulates to intermediate between the two levels when they are not identical is reduced to cases like (2d). For example, meaning postulates would still be needed to relate Tom and Tim are identical twins to Tim and Tom are identical twins. But if principle (1) is in effect, no grammar is possible in which the two deep structures

(3) a. I say[+performative] to you that prices slumped
b. Prices slumped

correspond to the single unambiguous concrete structure

(4) Prices slumped

with meaning postulates mapping both deep structures into a single abstract structure. This argument is used implicitly by Ross(1970) to defend the performative analysis and assert that (3b) cannot be a deep structure of the unambiguous (4).

In theoretical frameworks where deep structures and abstract structures are not similar (e.g., various "interpretive" theories and transformationalized Montague Grammar), and which therefore contain a rich set of rules relating the two levels, those rules are likely to be less complex in that the cases where distinct deep structures need to be mapped onto a single abstract structure are confined to those of (2d), where there are always distinct concrete structures as well. For example, interpretive rules would never be needed to map into a single abstract structure the two deep structures

(5) a. John left and Bill left
b. John and Bill left

both of which underlie the single, unambiguous concrete structure

(6) John and Bill left.

Principle (1) would exclude grammars that treat conjunction in such a way. Stockwell, Schachter and Partee(1973) make implicit use of this reasoning as one of their arguments against the existence of grammars which have both phrasal and derived conjunction. The fact that there is additional, independent evidence against such grammars indirectly supports the principle.

The principle in (1) would also exclude grammars containing transformational rules such as Conjunct Scrambling. This hypothetical rule would derive from the deep structure <u>Tom and Tim are identical twins</u>, the concrete structure <u>Tim and Tom are identical twins</u> by reversing the order of the conjuncts in the subject noun phrase. But if a grammar containing such a rule were allowed, there would be two deep structure sources for the unambiguous <u>Tim and Tom are identical twins</u> (and also, of course, for <u>Tom and Tim are identical twins</u>). The principle excludes all such grammars, which is consistent with the belief that many transformational grammarians have that "scrambling" rules are to be excluded from grammars of natural languages.

Hankamer(1973) proposes an analysis of English comparative structures that requires two "thans": $\underline{than}_1$ and $\underline{than}_2$. $\underline{Than}_1$ is a conjunction and is followed in deep structure by a clause; $\underline{than}_2$ is a preposition and is followed in deep structure by a noun phrase that must be "a nominative or accusative corresponding to an underlying subject of an intransitive or object of a transitive respectively, but not ... the underlying subject of a transitive" (p.185). Under Hankamer's analysis both (7a) and (7b) are deep structures of the unambiguous (8):

(7) a. Alfie loves his family more $than_1$ $_S$[Alfie loves food]
b. Alfie loves his family more $than_2$ $_{NP}$[food]

(8) Alfie loves his family more than food.

A question arises as to whether (7a) can underlie (8), but since Hankamer necessarily derives (10) from (9), where the target of comparison is an adjective, it seems unlikely that the derivation of (8) from (7a) can be prevented without some extremely ad hoc condition.

(9) The administrators are more stupid than$_1$ $_S$[they are malicious]

(10) The administrators are more stupid than Ø malicious.

The grammar that Hankamer proposes is excluded by the principle in (1), and it is therefore of interest that independent evidence exists that such a grammar is not adequate.

To begin, some general remarks are in order. The bifurcation of the single word form <u>than</u> into two homophonous words arouses the gravest suspicions when the two words can occur in exactly the same environment with no detectable contrast in meaning, as in example (8). Secondly, the constraint quoted above is quite unlike other constraints that transformational grammarians have proposed. Finally, there is the serious problem of how to define formally the notions "nominative corresponding to an underlying subject of an intransitive," and "accusative corresponding to an underlying object of a transitive," when the noun phrase target of comparison stands in none of the usual grammatical relationships with any verb.

Apart from these problems, there are serious exceptions to Hankamer's constraint in two directions. In one direction we find ourselves able to question the target of comparison when it is the "underlying subject of a transitive:"

(11) Who did you say you saw John gobbling knishes faster than?

According to the constraint, when the underlying subject of a transitive is the target of comparison, it can only occur in the clause that follows <u>than</u>$_1$, and can never occur as the object of the preposition <u>than</u>$_2$. But elements of the clausal complement of <u>than</u>$_1$ can not be questioned, since such clauses are islands. Indeed, this fact provides nearly all of the motivation for the "two-than" analysis in English in the first place. Thus much of the support in favor of the analysis vanishes if (11) is grammatical.

In the other direction we find examples such as (12), where it is highly unacceptable to chop elements out of the noun phrase position that follows <u>than</u>$_2$, also in contradiction to expectations based on Hankamer's analysis.

(12) *What would Max prefer to give old clothes to charity more than?
(Cf. Max would prefer to give old clothes to charity more than money.)

The grave doubts raised about this analysis provide evidence in support of (1) as a viable principle since (1) would exclude the possibility of such an analysis on meta-theoretical grounds.

Another consequence of the principle is that it leads us to reject grammars of English in which the indefinite article <u>a</u> is derived from a feature on nouns (Postal(1966)), or from a separate segment dominating the feature [-definite] (Chomsky(1965)). Under either analysis we can motivate two deep structures for the unambiguous

(13) John wrote a book about Watergate.

In one such deep structure the verb <u>write</u> simply takes a relative

clause direct object, as shown schematically in (14):

(14) John wrote $_{NP}$[a book $_{S}$[which is about Watergate]$_{S}$]$_{NP}$

Since write is subcategorized for a direct object such as book, it necessarily takes a direct object relative clause with book as head noun. The rule of Relative Clause Reduction derives (13) from (14).

We can also motivate a deep structure for (13) in which write takes a direct object a book, and an oblique object about Watergate.

(15) John wrote $_{NP}$[a book]$_{NP}$ $_{PP}$[about Watergate]$_{PP}$

The fact that elements in the complement of write can be questioned

(16) How good a book did John write about Watergate?
What did John write a book about?

and the fact that write takes an oblique object

(17) John wrote feverishly about Watergate.

provide strong evidence that (15) is also a deep structure of (13). Note that if a verb such as burn or discuss is substituted for write, the equivalent of (16) is unacceptable, as the reader can easily verify, and significantly, neither of these verbs is subcategorized for an oblique object.

A grammar in which (13) has two deep structures is excluded by principle (1). Within the framework of Postal's or Chomsky's analysis of the indefinite article, there is no way to block either derivation of (13), so it appears that either those analyses or the principle must be abandoned. Fortunately, however, Perlmutter(1968) has given an analysis of the indefinite article that is superior on independent grounds to both the feature and segment analysis and from which it follows naturally that the derivation of (13) from (15) is blocked, that is, (15) is not actually a deep structure of (13). We must digress somewhat in order to see this clearly.

Consider the sentence in (18), which is entirely parallel to (13).

(18) John wrote his fifth book about Watergate.

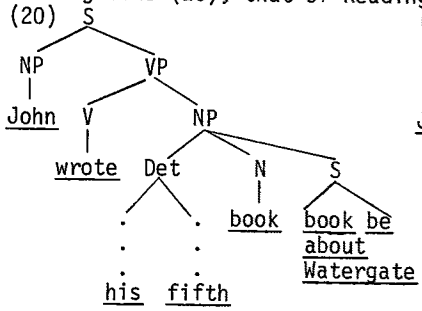
In its written form (18) is ambiguous:

(19) Reading (I): John's Nth book (for any N $\geq$5) is his fifth one about Watergate. (The "scope" of fifth is book about Watergate.)

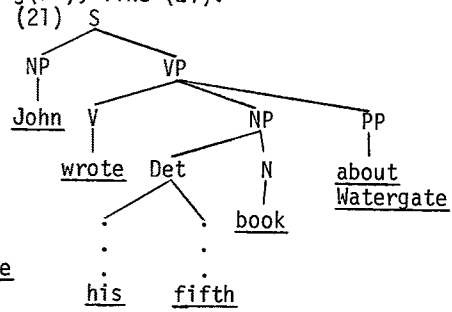
Reading (II): The fifth book John wrote was about Watergate. (The scope of fifth is book.)

The deep structure of (18) that corresponds to Reading(I) is something like (20); that of Reading(II), like (21):

(20)



(21)



The parallel between (14) and (20), and between (15) and (21) is self-evident.

In its spoken form (18) is not ambiguous. With heavier stress and higher pitch on book than on fifth, Reading(I) is obtained; with heavier stress and higher pitch on fifth than on book, Reading(II) is obtained. (In addition, four other readings are possible in which contrastive stress is placed either on fifth or on book.)

If, as in (22), the numeral five is substituted in the Determiner of (18) we expect a pair of readings analogous to those indicated in (19).

(22) John wrote five books about Watergate.

One of these, corresponding to Reading(I) of (19), is paraphrased as:

(23) Of the five or more books that John wrote, five are about Watergate. (The scope of five is books about Watergate.)

The other, corresponding to Reading(II) of (19), should be paraphrasable as:

(24) Of the exactly five books that John wrote, all five are about Watergate. (The scope of five is books.)

But surprisingly, (22) never has the meaning expressed in (24). If such a reading is forced by placing heavier stress on five than on books in (22), only a contrastive reading is obtained.

It is unclear how to account for this difference between (18) and (22). Apparently there are obligatory rules of stress and pitch assignment that are sensitive to the scope of elements in the Determiner. For some reason these rules are prevented from assigning heavy stress and high pitch to numerals, so the derivation of (22) from a deep structure of the form of (21) with a numeral in the Determiner is blocked because of the failure of an obligatory rule to apply. The rules assigning contrastive stress and pitch, however, may apply independently, and this is the only reading available of (22) when the numeral five receives high stress and pitch.

We are now prepared to account for sentence (13) without violating the principle in (1). Under Perlmutter's(1968) analysis, the indefinite article a is derived from a deep structure containing the numeral one, which assumes the form a just in case it is unstressed. Potentially (13) has two deep structures, which look like (14) and (15) except that one is substituted for a. In (14) the rules of stress and pitch that are sensitive to scope do not assign stress to one and so it surfaces as a. In (15), however, stress should be assigned to one, but this cannot happen because of the general constraint against assigning non-contrastive stress to numerals. Thus (15) is not a possible deep structure of (13), and a grammar containing Perlmutter's analysis of the indefinite article does not violate principle (1) insofar as present considerations are concerned.

In confirmation of these conclusions we note that the single reading we get of (13) corresponds to the deep structure (14), which corresponds to Reading(I) of (19), namely that John wrote one or more books, and one is about Watergate. Sentence (25), with heavy stress and high pitch on one, can only have a contrastive reading, as our line of reasoning would predict.

(25) John wrote ónе book about Watergate (not five).

One final question remains, and that is how do we account for examples (16), which cannot have deep structures similar to (14) because of the Complex Noun Phrase Constraint. In fact, examples such as (16) have deep structures similar in form to (15), with the

numeral one substituted for a. The syntactic rearrangement produced by the Question Transformation allows the one to be unstressed and ultimately reduced to a. The reader can confirm that the only meaning available in (16) is where the numeral, i.e., the indefinite article, has only book in its scope. Thus the question What did John write a book about has a reading something like: "John wrote one single book and I am asking what that book is about." The inappropriateness of asking What did Hemingway write a book about follows from these considerations.

In conclusion, then, the principle in (1) is of interest to transformational grammarians on a meta-theoretical basis, and on the grounds that it leads us to reject inadequate analyses such as Hankamer's, and to accept superior analyses such as Perlmutter's.

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A UNIFORM FORMAT FOR GRAMMATICAL DESCRIPTIONS

Edward L. Blansitt, Jr.

The University of Texas at El Paso

Anyone who has done a detailed cross-linguistic study of some grammatical category will have undoubtedly experienced the frustation that comes from examining completely an apparently comprehensive reference grammar only to find that the information he is searching for is not included. Many a grammar has no index and the table of contents is frequently too general to be useful. In some cases a grammar will have a section ostentatiously dedicated to a particular category when in reality there is more information about that category coincidentally in other sections. After such a frustrating experience one certainly must wonder whether there is not some way that work on grammatical typology and universals can be made less time-consuming.

Grammatical categories may be studied cross-linguistically in a much more efficient manner if it is possible to isolate the pertinent grammatical fragments so that data not pertinent to the investigation can be excluded. This ideal can be achieved if a sufficient number of grammars are written with a common table of contents and each grammarian carefully includes all the pertinent information in the appropriate section. Also, in some cases, when a relatively complete grammar is already available for a language, a comprehensive index of the extant grammar could be prepared in the uniform format.

I first proposed a uniform format for grammars in a lecture on tagmemics and language universals --the fifth in a series of five lectures on tagmemics-- at the Centro de Lingüística Hispánica of the National University of Mexico on November 26, 1971. The format is really designed for the reader of a grammar whose purpose is to study the entire structure of the language described, not just fragments of the grammar. One of the exasperating experiences which such a reader has with many grammars is that he finds examples given in the first sections which involve structures which are not treated until later. The format thus begins with the smallest grammatical structures --word-level structures-- and proceeds level by level through phrase and clause. Backlooping is postponed until all levels of structure have been described, so that noun phrases are described on the phrase level minus such backlooping structures as relative clauses. In this way, the reader who moves progressively from the first to the last section should never encounter an undescribed structure in any example.

The major headings in the outline for grammars are 1) word-level structures, 2) phrase-level structures, 3) clause-level structures, 4) nuclear participant roles, 5) derivational morphology,

6) varying-level phenomena, 7) sentence structures, 8) structures of functional equivalents, 9) backlooping, 10) context-dependent sentences, and 11) intersyntagmemic relations. In a number of the subsections a hedge against incompleteness is the final category labelled "others." On the word level, there are separate subsections for verb, adjective, noun, and so forth, for inflection and compounding. The actual order in which the given subclasses of structures on a particular level is given is sometimes arbitrary, but adjective phrases are treated before noun phrases so that the former may be included in the description of the latter. The list of adverbial phrases, i.e., phrases which manifest a peripheral clause-level tagmeme, is very long and there seems to be little reason to put them in a particular sequence. Clause-level structures are divided into two subsections: 1) emic clause types and 2) etic clause types. In the first of these, several clause types are shown which are assumed to be emic universals and others may be. Under etic clause types are listed possessive, copulative, existential, and meteorological, which represent different emic types in different languages. Possessive clause is intransitive in Aymara and may be in Tarahumara, as the verb is derived from the possessed-referent noun; it is also intransitive in Navajo, as possessor and possessed are combined in the subject and occur with an existential predicate. Possessive clause is unitransitive in English and stative in Swahili.

Although some of the categories are assumed to be universal, some others are admittedly not universal. If some category such as noun inflection does not occur in the language being described, a simple statement to that effect is to be entered in the appropriate section (I.B.1).

The most recently added section is No. IV: nuclear participant roles. These correspond approximately to the propositional cases of case grammar; the adverbial phrases, which manifest peripheral clause-level tagmemes, correspond to the case-grammar modalities. It is assumed that there is a one-to-one correlation between peripheral clause-level tagmemes and semantic values, but such nuclear clause-level tagmemes as subject, direct object, and indirect object may correspond to different participant roles. One problem is how to handle dual or multiple participant roles[1]. The "transferor" role of a voluntary transfer can apparently be eliminated --and is not shown-- by combining 'agent' and 'source'; "transferee" represents a combination of 'beneficiary' and 'goal.' There are also some nuclear tagmemes which may correspond to none of the roles inventoried: the direct object of <u>avenge</u>, as in <u>Frank will avenge Bill's murder</u>, which can also be passive subject in <u>Bill's murder will be avenged by Frank</u>, corresponds semantically to "motivating event." Similarly, with the etymologically related verb <u>vindicate</u>, as in <u>Frank will vindicate Bill</u>, it is difficult if not impossible to assign the direct object to one of the inventoried roles; to assign it to 'beneficiary' would at least be controversial. In <u>Frank will get even with Paul</u>, <u>Frank</u> cannot unequivocably be assigned to the participant role 'agent,' as <u>getting even</u> does not necessarily involve an "action," and <u>Paul</u> might be 'patient,' 'experiencer,' or

--in the negative sense-- 'beneficiary.' Generative semanticists may also find an equally serious problem with the direct object of avenge; if the meaning of "avenge" is "cause injury to the person who is/was responsible for," then the derivation of the "surface structure" from the corresponding "logical structure" may involve violation of John Ross' "Complex NP Constraint."[2]

Another recent change in the format has been the removal of derivational morphology from the section on word-level structures to a new section following clause-level structures and now also following nuclear participant roles. The change has been made in recognition of the fact that stems may represent structures belonging to a higher level than word level. The adjective, loose-knit modifier, three dimensional, for example, consists of the restricted noun phrase three dimensions + the relator (derivational affix) -al.[3] The type of backlooping involved in derivational morphology is of a much more restricted kind than that which involves whole noun phrases or whole clauses and so has not been included in the section on backlooping: section IX.

A special section is also dedicated to such features as negation, causation, tense, mood, and aspect, which are realized in different languages on different levels of grammatical structure. Another section is dedicated to sentence structure, where contrasts between assertion, question, command, and exclamation are included. The concept of sentence is not viewed --as it is in traditional tagmemics and in systemic linguistics-- as a layer of clause combination. The present outline does not include a special section for non-clausal context-independent sentences, which are much more common than is generally thought. Such sentences could, of course, be included in subsections of the appropriate sentence type; therefore, a sentence such as Me and my big mouth --which means something like I shouldn't have said anything-- could appear in a non-clausal subsection of declarative sentences and How about some coffee? --which can mean How about my supplying you with some coffee? as easily as How about your supplying me with some coffee?-- could appear in a non-clausal subsection of interrogative sentences.

For a uniform format for grammars to succeed, definitions of the terms used should be unambiguous. Definitions are to be included in a monograph which is in preparation. The definitions may be partly based on formal and relational criteria, but the inclusion of semantic criteria is inescapable. Structure types are based on recognition of a semantic prototype; a prototype bitransitive clause may be characterized semantically as a voluntary transfer, not specifically temporary and not specifically involving exchange, intermediary, or motion[4] and the transferor will be identified as grammatical subject, transferee as indirect object, and transferred as direct object. Structures which are formally identical to the prototype bitransitive clause can then be identified as bitransitive on the basis of form alone. Once the definitions have explained the concepts, the actual use of the terminology presented is optional. If someone wishes to substitute "sentence structures" for what we have called "clause-level structures" and "performatives, clefting, and passivization" for "sentence structures," nothing is lost. If

instead of "bitransitive clause" someone prefers "ditransitive clause," "double-transitive clause," "a subclass of three-place predicates," or "a subclass of double-patient sentences," there will be no problem. A researcher will identify the grammar fragments needed by section numbers, not by terminology.

The format is essentially model-neutral. The order of presentation is dictated by concerns which are not themselves model-oriented. The original purpose was to propose a uniform format for tagmemic grammars, but this was based on the apparently false assumption that such a format had to have a clear theoretical orientation. The format may have no theoretical implications; it does bring out the fact, which perhaps no one has doubted anyway, that there are certain empirical realities of language structure which are theory-independent and for which the only thing original which a new theory can offer is just a new name. Of course, if the grammatical categories are defined in such a way that one and only one uniform-format grammar is possible for any given set of language data, this will constitute a statement about the structure of language of the strongest possible kind.

In spite of an essential model-neutrality of the proposed uniform format for grammars, work towards such an end in itself constitutes rejection of a current fad in linguistics: speculation about unreality. As in other grammars, the structures described will presumably be well-formed and free of false starts and other blunders; in this sense it may be said that the intention is to describe the speaker's grammatical competence. The format makes no allowance for describing a phantasy such as a linguistic competence which generates sentences by moving from a deep structure to a surface structure by application of some sort of derivational rules. The format does recognize grammatical meanings, elusive though they may sometimes be; probably all speakers of English recognize or can be easily made to recognize that the subject Mary in Mary slapped Frank and in Mary kicked Frank share a common meaning not shared by the same subject in Mary likes Frank. Understanding how a speaker moves from meaning to sound in encoding linguistic messages must be one of the primary goals of linguistics, but preoccupation with this problem must not cause us to lose all concern for describing the grammatical structure of language. Understanding the different subsystems of language is, in fact, prerequisite to understanding how these subsystems enter into the processes of encoding and decoding linguistic messages. On the other hand, speculation about such unrealities as the fictitious deep structure of the fictitious grammar of the fictitious competence of the fictitious ideal speaker-hearer is only as pertinent to linguistics as the study of the anatomy of unicorns and gremlins is to zoology or a map of Never-never Land is to geography. The uniform format for grammars is designed to describe that part of the real world which is human language.

There are applications of a uniform grammatical format in addition to solving problems of researchers in grammatical typology and universals. For any two languages described in the format, a contrastive grammar may be produced immediately by simply comparing each section of the two grammars.

One may ask if it would not be just as useful to prepare a check-list of topics to be covered in a grammar so that all writers of grammars might ascertain that each topic had adequate coverage in his grammar; the same check-list could also serve as an index. Even if a hybrid terminology were adopted with entries from all the principal schools of linguistics and linguists from the different groups accepted the compromise in terminology, a number of problems would still be unsolved. If an alphabetical order were used on the check-list, the order would vary from language to language. In addition, some of the other advantages of the proposed uniform format, such as the order of presentation for easy reading, might still be lacking.

In specific relation to the uniform format, a basic sentence list has been prepared which is designed to allow, when translated into any given language, adequate information to construct a uniform-format syntax. It is also designed to provide all the basic information on word-level typology. Some further refinements are being made in the basic sentence list. There are two main problems: 1) how to get informants to use the dominant order in the target language when the dominant order of the interview language is the same as an alternative but non-dominant order in the target language and 2) how to discover contrasts in the target language which do not correspond to any contrast in the interview language. The solution to the first problem seems to be to check the informant's reactions to certain permutations performed on his responses; the second problem is somewhat more difficult to solve, though not impossible. Hopefully, the uniform format for grammars with a basic sentence list keyed to it will permit construction of grammars of endangered languages which might otherwise never be described.

NOTES

1. M. A. K. Halliday shows single, dual, and triple participant roles ("Notes on Transitivity and Theme, Part I," in Journal of Linguistics 3:37-81).

2. John Robert Ross proposed the Complex NP Constraint in his 1967 MIT dissertation, "Constraints on Variables in Syntax," pp. 66-88 (distributed by Indiana University Linguistics Club). James D. McCawley rejected any possible lexical item meaning "kiss a girl who is allergic to" on the basis that it would involve a violation of Ross' constraint ("Prelexical Syntax," Monograph Series on Languages and Linguistics XXIV:19-33 (1971), ed. by Richard J. O'Brien, S.J., Georgetown University Press).

3. Peter H. Fries analyzes loose-knit modifiers such as three dimensional as consisting of an axis manifested by a restricted noun phrase and a relator manifested by a derivational affix. (Tagmeme Sequences in the English Noun Phrase, p. 77, Summer Institute of Linguistics, 1970). I had also independently reached the conclusion that derivational affixes were word-level relators

("Grammatical Constituents," in Studies in Linguistics XXI:11-25 (1969-70)).

4. I used the semantic description of a prototype structure as a basis for identification of the bitransitive clause in "Bitransitive Clauses," in Working Papers on Language Universals # 13:1-25.

APPENDIX

UNIFORM-FORMAT GRAMMAR OUTLINE

I. Word-level Structures
   A. Verb
      1. Inflection
      2. Compounding
   B. Adjective
      1. Inflection
      2. Compounding
   C. Noun
      1. Inflection
      2. Compounding
   D. Pronoun
   E. Adverb
   F. Relator
   G. Others (1 & 2 above for each where applicable)

II. Phrase-level Structures
   A. Verb Phrase
   B. Adjective Phrase
   C. Noun Phrase
      1. Subject
      2. Direct Object
      3. Indirect Object
      4. Directional
      5. Nominal Complement
      6. Others
   D. Adverbial Phrase
      1. Temporal
      2. Iterative
      3. Terminative Durational
      4. Non-terminative Durative
      5. Locative
      6. Applicational
      7. Instrumental
      8. Limitational
      9. Comitative
      10. Benefactive
      11. Ablative

12. Topical
13. Qualitative Manner
14. Specificational Manner
15. Purposive
16. Causal
17. Conditional
18. Concessive
19. Others

III. Clause-level Structures
- A. Emic Clause Types
  1. Intransitive Clause
  2. Unitransitive Clause
  3. Bitransitive Clause
  4. Stative Clause
  5. Motional Clause
  6. Stative-transitive Clause
  7. Others
- B. Etic Clause Types
  1. Possessive Clause
  2. Copulative Clause
  3. Existential Clause
  4. Meteorological Clause
  5. Others

IV. Nuclear Participant Roles
- A. Agent
- B. Experiencer
- C. Patient
- D. Beneficiary
- E. Place
- F. Time
- G. Source
- H. Goal
- I. Others

V. Derivational Morphology
- A. Verb
- B. Adjective
- C. Noun
- D. Adverb
- E. Others

VI. Varying-level Phenomena
- A. Negation
- B. Causative
- C. Tense, Mood, and Aspect
- D. Reflexive and Reciprocal
- E. Others

VII. Sentence Structures
- A. Declarative
- B. Interrogative
  1. Corroborative
     - a. Plain
     - b. Tag
     - c. Tag-answer

- 2. Informational
- C. Imperative and Exhortative
- D. Exclamatory
- E. Focus-complement
- F. Passive and/or Indefinite Actor
- G. Others

VIII. Structures of Functional Equivalents
- A. Coordination
- B. Apposition
- C. Reduplication

IX. Backlooping
- A. Backlooping Phrases
  - 1. Determiner
  - 2. Close-knit Modifier
  - 3. Completion
  - 4. Restrictive Modifier
  - 5. Others
- B. Backlooping Clauses
  - 1. In Adjective Phrases
  - 2. In Noun Phrases
    - a. Completion
    - b. Relative
      - i. Restrictive
      - ii. Non-restrictive
    - c. Others
  - 3. In Clauses
    - a. Noun Clauses
      - i. Subject
      - ii. Direct Object
      - iii. Directional
      - iv. Others
    - b. Adverbial Clauses
      - i. Temporal
      - ii. Iterative
      - iii. Durational
      - iv. Locative
      - v. Manner
      - vi. Purposive
      - vii. Causal
      - viii. Conditional
      - ix. Concessive
      - x. Others

X. Context-dependent Sentences (Substitution and Deletion)

XI. Intersyntagmemic Relations

TOWARD AN ECOLOGICAL DICTIONARY OF ENGLISH

Adam Makkai
University of Illinois at Chicago Circle

0. Introduction

The purpose of the present paper is twofold: First, I would like to point out that as a nonnative speaker of English I have been frustrated for over two decades even with the best of our currently available dictionaries for their inconsistencies in marking the range of words, their frequency, their synonyms, their applicability in certain contexts, such as idioms and the like, and, second, I would like to raise the question whether there is any adequate modern theory available today that could somehow remey the situation by stimulating the creation of a new kind of dictionary.

Some years back I looked up once the entry *maverick* in five leading dictionaries. The definition conformed with what I knew about the term, and one dictionary even gave me the story how it arose. Mr. Maverick, a long time ago, for humanitarian reasons in Texas, wouldn't brand his cattle in conformity with the prevailing customs. Thus 'loner', 'noncomformist' makes indeed good sense as the definition of that term. Two of the five dictionaries consulted also gave the marking US which made me think that this term was only used in the United States. Two weeks later I happened to be in London, England and heard the television newscaster characterize a rebel Member of Parliament thus: *That indomitable old maverick voted against the Prime Minister's program.* 'Aha' - I said to myself, *maverick* has been borrowed over into British English. And apparently so it has. No dictionary has made that adjustment in its marking of the entry, however. Then I tried to look up the entry *hominy grits* to see how it was treated. It said in all five dictionaries US and some gave descriptions how it was made and of what ingredients. I subsequently tried to order it in a Boston restaurant, then in New Haven, Connecticut, and later in Chicago. Most of the time the waiters simply started me in the face, only one Chicagoan had the nerve to say 'Hey, Mister, you gotta be kiddin', we ain't in the State of Missisippi aroun' here! ' And so it goes. Americanisms are not necessarily only Americanisms, and when they are, they are by no means universally understood in the United States. On the other hand, no dictionary that I ever came across, at least so far, indicates, if a term is exclusively British either. It is almost as if the British forms were regarded as the 'unmarked' ones which every one has the duty of recognizing with the Americanisms somehow marked for regionalism. Consulting the *American Heritage Dictionary* one learns that the term *perambulator* is 'chiefly' British. But what does the term *chiefly* really mean? What is the margin of usage of *perambulator* (or *pram* for short) in the United States? Zero? Limited to the urban intellectual elite of the East Coast? Only Boston? What about Canada? Australia? New Zealand? Have New Zealanders borrowed the Americanism *baby carriage*, or do they walk their infants in British *prams?* Of course one generally believes that Commonwealth countries use the British term predominantly, but as the case of *maverick* clearly shows, that is not necessarily so either. I have asked many

Australians about *pram* and *baby carriage* and the vote was amazingly even: Some knew that *baby carriage* was an actually borrowed 'Americanism' but insisted that they used it regularly instead of *perambulator-pram* which some characterized as 'limey'. If, in the *American Heritage Dictionary*, one tries to look up how to say 'commit a social blunder in speech' there is literally no way to find the appropriate expression since neither *social* nor *blunder* is cross-referenced with any such expression. It is merely by chance that one finds *put one's foot in one's mouth* under the entry *foot*. There is no indication whether it is an Americanism, a Commonwealth expression of common currency, or what. It turns out that in England one says *he really dropped a brick, didn't he?* when they mean to say that somebody committed a social blunder. Marginally, I have learned, some British speakers might say *he put his foot in it* but not **in his mouth*; that reminds them of what a contortionist does in a circus. Similarly, I tried *he dropped a brick* on countless Americans only to find that with the exception of an elderly lady living in Boston nobody got the picture; most were thinking of a clumsy mason letting go of his building blocks. What would be preferred in Australia, New Zealand, or Fort Colombo, Sri Lanka, is anybody's guess. 'If uncertain, stick to proper British English' - a friend once advised, but then he was speaking in self-mockery.

But there are other problems as well. Technical terminology can sometimes grow so fast that it is impossible to keep abreast of it, even if it is the product of an industry relatively well introduced to the public. Let us consider for a minute what the space industry has done to American English during the past fifteen years. *Dog house* in Space English means 'a small instrument package attached to the tip of a rocket.' *Fire in the hole* is the name of a complicated manoeuvre in space. *Apolune* and *perilune* differ from *apocynthion* and *pericynthion* for complex technical reasons (cf. Makkai 1973, Introduction), and whether such prefixing will ever be necessary with the name of the planets in the Solar System is anybody's guess. My *Dictionary of Space English* (1973) carries over 3,000 definitions and 70 illustrations and is considered as the bare minimum coverage of the most important expressions that have arisen during the past 15 years. How, if at all, should the corpus of that glossary be incorporated in a larger, more comprehensive dictionary of English? Where do subdictionaries belong in an ecological treatment of the 'entire' English word stock?

The next question I wish to bring up is this: What theory, or theories of language, if any, can currently exisisting dictionaries be attributed to? I would welcome serious professional bids claiming that such-and-such a dictionary has actually been compiled according to 'Neo-Bloomfieldian' principles, or, that this other dictionary here exhibits the tenets of Kenneth L. Pike's tagmemics, or that this brand new dictionary here is, in fact, the product of Transformational-Generative Grammar. But this is never claimed. And for excellent reasons: By and large, all currently available dictionaries are the product of 19th century philology. (This may, incidentally, also be the reason why we have any workable dictionaries at all.)

What would a 'scientifically' constructed Transformational-Generative Dictionary look like? It would, probably, be a huge feature inventory showing the precise semantic and phonetic features of every entry. All entries that start, say, with the prefix *trans-* would have to repeat each time all of the phonetic features that go into the sounds /trænz/, and having seen and studied Chomsky and Halle's *The Sound Pattern of English* (1968) we all know what some feature specifications can look like. I think that if we imagine 100 tomes of the size of the unabridged *Oxford English Dictionary*, we may get through a third of the English alphabet in that kind of a notation. I consequently rather doubt if a fully specified TG dictionary of any natural language will ever be written.

There is another theory, however, though less popular and less glamorous, which

may be able to point the way to a reasonable overhauling of traditional dictionaries without becoming useless for human users. This theory is stratificational grammar.In his *Outline of Stratificational Grammar* (1966) Lamb suggested that a stratificational account of the word stock of English would be divided into four sections, called by him the **semicon**, the **lexicon**, the **morphicon**, and the **phonicon**. The basic idea is a relatively well known one, already mentioned in other terms by Householder in his essay on 'Linguistic Primes' (1959) and can be paraphrased briefly as follows:

(i) The number of phonetic features (or, on a higher level, in structuralism, the 'classical phonemes') can be compared in terms of rank and magnititude to the symbols on a typewriter, varying between 44 and 88. According to some estimates the number of phonetic features English phonemes can be described by is a little more than a dozen. This is what may be viewed as the **phonicon** of a language, i.e., the inventory of its sounds.

(ii) By multiply re-investing these sounds in certain sequences, one arrives at the much more heavily populated **morphicon** of the language. The number of morphemes is, on the average, up in the thousands, or even the tens of thousands. Some morphemes have independent lexemic status in the language, some only occur as building blocks of lexemes. Thus the morphemes *hot* and *dog* are elements in the **morphicon** of English, but in the compound *hot dog* 'frankfurter in a bun' we find them re-invested devoid of the lexical meanings of *hot* and *dog*, respectively, merely as morphemic shells.

(iii) Thus by virtue of the multiple reinvestment of the morpheme stock we arrive at the lexeme stock of a language, and the number of lexemes may be up in the hundreds of thousands. Needless to say, many of the morphemes are simultaneously lexemes as well, but most lexemes are polymorphemic in English, a language heavily suffused with foreign elements from various periods of its history.

(iv) Yet the number of meanings a language deals with is still not arrived at by the listing of all of its lexemes, because some socially categorized and commonly used meanings are expressed by an arbitrary configuration of lexemes. Thus when we say to some one *why don't you pass me the sugar?* or if a secretary says over the phone *may I ask who's calling?* we do not mean 'what is the cause of your non-yielding of the sugar?' or 'am I permitted to inquire as to your identity?' – but rather both instances are a socially agreed upon realization of the sememe of ***politeness***. Foreigners must learn these nuances by trial and error, but their success in the English speaking world is measured – perhaps largely unconsciously – on the part of native speakers precisely by how efficiently they manipulate these hidden, yet socially highly relevant, sememes of the language. For a more detailed discussion of some of these **idioms** see Makkai 1972. The **semicon**, then, is the most densely populated section of a dictionary, as it houses all the meanings of monomorphemic lexemes, polymorphemic lexemes, and meanings not overtly present in the text or utterance at hand.

Now the way stratificational grammar helps in producing a ***realistic dictionary*** should be fairly obvious. First, we recognize the stratification of language as applied to one dialect of one language at a time, and then we project the image so gained to encompass all the dialects of the English language. The result is an **Ecological Dictionary of English**, that is the **EDE**. We need a small alteration in spelling in order to express this fact. Lamb's **phonicon**, **morphicon**, **lexicon** and **semicon** could be respelled with an *e* in the middle to indicate that we are dealing with the **ecological treatment of the sound, morpheme, and lexeme stock in order to build a sem*e*con of the language.** (As we

all know both *ecology* and *economy* ultimately derive from Gk οἶκος 'house').

1. Purpose of the EDE: The Present Situation

Nothing significantly new has happened in lexicography since the first printed dictionaries after Gutenberg invented the printing of books. There is, to be sure, a steady stream of small step-by-step technical, or detail improvements in almost every new dictionary of any language–but, essentially, they all dance to the same tune. One could characterize all familiar dictionaries with the invented acronym ***HABIT***. The word stands for 'Heterogeneous Alphabetical Inventory'.

We are all familiar with ***HABIT*** dictionaries: they start with the letter *A* and take us to the letter *Z*. Some offer modern phonological transcriptions of pronunciation, some do not. Some offer detailed etymological information, some do not. Some include the brief biographies of famous American writers and political figures, others do not. Some carry illustrations in the fashion of the *Petit Larousse Illustré*, others do not. Better ***HABIT*** dictionaries will carefully indicate if a term is an Americanism, as is done in the *Oxford English Dictionary*, but one never finds a marker indicating where else that "Americanism" is used and understood, and what part of the USA it is restricted to, if any. Pronunciations are usually given for one dialect only. There is usually no indication given in ***HABIT*** dictionaries how the words *dog, money,* and *London* are pronounced in London, England; Essex, England; Edinburgh, Scotland–to name just a few British dialect areas, and say in Boston, Massachusetts; Birmingham, Alabama; and Chicago, Illinois.

HABIT dictionaries list everything, according to the established habit, alphabetically. The harder editors try to be complete, the more dead ballast they have to carry in dictionaries meant, as we are usually told, for that nondescript entity known as the "general educated public". Hence constant problems arise: just how much chemical terminology should a good dictionary carry? How much of modern slang? How many astronomical and modern space terms should be found in it? How many medical terms–and so on.

Without going into further detail, I believe that the foregoing paragraphs give a reasonable summary of some of the major problems of the usual dictionary. Let us now take a look at how it can be reorganized, to accommodate everything where it is needed and truly useful to the "general educated public" as well as to the not-so-well-educated-public; after all, the purpose of a dictionary should be, among other things, to offer a way for foreigners and uneducated people to acquire a better use and understanding of the language. There is nothing wrong with a dictionary serving pedagogical purposes as well, if that can be integrated into its other functions simultaneously.

2. What Would a TRUE Dictionary be Like?

The proposed *Ecological Dictionary of English (EDE)* would be a ***TRUE*** dictionary. The letters of this acronym stand for 'Thesaurus Rendered Updated Ecologically'.

It is my contention that the only really significant change in dictionary making occurred when Roget published his first *Thesaurus* in the latter decades of the 19th century in England. In Roget's *Thesaurus* and subsequent editions thereof, the user finds a handy nest of synonyms worked out carefully for each entry. These *semantic nests,* as I propose to call them, do fairly good justice to the entries covered, as far as the knowledge and patience of the original compilers permitted. But the markings in Roget are, again, inconsistent. The many attempts at modernization of the dictionary have disturbed Roget's original mode of organization and the additions are not truly systematic.

The question arises: why do I find alphabetization a negative rather than a positive feature, and what else do I have to offer instead of it? Alphabetization in itself is not all bad, tyrannical though it may seem, since we are used to it, and usually we know what letter an entry that we are looking for starts with. But have we not all noticed how certain sections of a ***HABIT*** dictionary get dirty and worn, with others barely touched? The reason is that entries used by the general public are clustered in certain portions of the dictionary and this clustering sometimes agrees with the essentially arbitrary order of the alphabet, and sometimes does not. (Note that a student, or a foreigner will look up different entries in a dictionary than a specialist in a certain technical field.)

What we, therefore, need is a primarily unalphabetized dictionary in which the entries–resembling Roget's synonym groups, but improved–would occur *not in the alphabetical order but in their order of frequency in the spoken and the written language.* Such word frequency lists are easily available. One of the best known ones is E. L. Thorndike's *The Teacher's Manual of the 20,000 Most Frequent Words in English,* published in New York in 1920, republished and revised in 1952 with the co-authorship of Lorge. (The main problem with the Thorndike-Lorge word list is that no indication is given as to a certain word's frequency as it relates to its position in the sentence. E.g., the entry *man* has a 1A marking; it is among the most frequently used English words, but is this based on the word *man* as a noun, as in *John is a nice man,* or based on the verb *man* as in *John will man the post?* One suspects, of course, the noun to be more frequent, but TL completely ignore even elementary syntax.) Additionally, the RAND Corporation in Santa Monica, California, carries the last forty years or so of *Time* magazine on computer tape, and could produce a count program yielding a readable print-out of the frequencies observed.

A ***TRUE*** dictionary would start, say, with a set of words such as *yes, no, I, you, man, go, get, put, take, run, make, water, eat, food, buy, where, who, today, tomorrow,* etc. But how, then, would the user find the entry he is looking for? Let us imagine that the entire corpus consists of these nineteen words. Also imagine that they occur in the order of frequency listed above (*yes* the first, *tomorrow* the last entry). At the end of the dictionary there would be an alphabetical indicator, without any definitions. In the present case it would look as follows:

BUY 15, EAT 13, FOOD 14, GET 7, GO 6, I 3, MAKE 11, MAN 5,
NO 2, PUT 8, RUN 10, TAKE 9, TODAY 18, TOMORROW 19,
WATER 12, WHERE 16, WHO 17, YES 1, YOU 4.

Next to the serial number, of course, there would be a page number indicator as well. It would be open to discussion whether this alphabetical indicator should be at the beginning or at the end of the dictionary; there are acceptable arguments for both solutions.

3. The Two Faces of HABIT: The Ecology of Expression and the Ecology of Content

Let us take as an arbitrary example some of the meanings that the verb *take* can enter into in modern standard American English:

be taken aback	'be startled'
take after somebody	'resemble'
take back	'renege on promise'
take down	'write upon dictation'
take someone for something	'consider to be'
take in (1)	'be able to hold physically'
take in (2)	'absorb mentally'
take off (1)	'start to go at high speed'
take off (2)	'become airborne'

take on (1)	'engage in combat'
take on (2)	'employ new workers'
take on (3)	'assume (new) responsibilities'
take over	'assume control'
take up	'begin to study'
take someone up on something	'make someone keep his promise'
take something up with someone	'discuss'
take to someone	'have a liking for'

These seventeen examples are all verbal in nature. On the nominal side we find:

intake	'capacity'
take-off (1)	'parody'
take-off (2)	'period in which plane becomes airborne'
take-over	'coup d'état'
uptake	'mental ability, capacity'

There are more, to be sure. As we can see these examples deal with *take as a matter of expression.* In other words, here we have listed some of the occurrences of *take,* whatever they mean. (Similarly, by the way, one would indicate in a ***TRUE*** dictionary that the prepositional-adverbial particles *back, down, in, off, on, over, up,* etc., co-occur with these expressions headed by *take.)*

How would the content side be organized, by contrast? Let us take the meaning 'happen' as an example. We would list, thesaurus-like, the following expressions all of which share, denotatively, if not connotatively, the meaning of 'happen': *come about, come out, be up, fall out, work out,* and *turn out.* Test sentences (examples) would be provided to show the validity of the choices. The matrix sentence in this case would be *this is the way it X-d.* Thus we have, for *this is the way it happened,* a possible *this is the way it came about, this is the way it fell out, this is the way it turned out,* and *this is the way it worked out,* but for *be up,* as in *what's up?* 'what's happening', we would have to restrict the form to the present tense, as we do not hear **what was up?*

One of the shortcomings of a traditional thesaurus is that it fails to give sufficient specification as to when two words can be used as synonyms and when not. Thus the expression *this is how it fell out* certainly carries the meaning 'happen' but it also carries the connotative meaning of 'by chance'. *This is how it worked out* carries the implication that the problem evolved in its own manner and resolved itself without too much outside interference.

In ***TRUE***, then, we would do exactly what the human brain does when a native speaker of English stores this kind of information. We would cross-refer the user from any occurrence of the form *take* to any and all the possible meanings that have to do with 'seize, take, grab,' etc., but such a dictionary, being truly ecological in orientation, would also show where the limits of such correspondences are.

4. The Morpheme-Lexeme Ratio

English has a much smaller number of morphemes than it has lexemes. No currently available dictionary makes that distinction explicitly. There is, to be sure, only one *trans-, re-, de-, in-, con-,* etc., in English, but because of the unpredictability of some of the resultant meanings, (cf. *translate,* versus *transfer)* it is hard to assign a constant meaning to a single form *trans-.* We know, of course, that in Latin it meant 'across'. An ecological dictionary would thus establish for the first time a reliable *morpheme reinvestment count.* This would show which are the most heavily exploited forms in the language that occur in more, longer forms with a unique meaning.

5. The Retro-Depository: ***HARD***

The letters stand for 'Heterogeneous Alphabetical Retro-Depository'. This serves a function whose use is 'hard' to see at first glance, hence its name. It spells the whole dictionary backwards (not the words themselves, only their order) with the last letter of each word being considered first. In Europe several such so-called *a tergo* ('backwards') dictionaries have been written and put to interesting use. For one thing, they would produce the most complete rhyme dictionary ever written. Second, the retro-depository would show the most frequent suffixes and the stems they adhere to, such as *-ness, -dom, -ship,* and *-hood,* for instance. Both the retro-depository and the straightforward alphabetical look-up could be made into appendices to the main dictionary itself.

6. Indexing for National Areas (NAs)

English is spoken today as national language in England, Scotland, Wales, and Northern Ireland (the United Kingdom), the Irish Republic, Canada, the United States, Australia, and New Zealand. If a form in the ***EDE*** is universally recognized and used in all of these ***NAs,*** (e.g., the verb *go,* it would be marked ***NAun,*** meaning 'universally understood in all National Areas'. If, by contrast, a form is restricted to the United States (e.g., *gerrymander* or *hominy grits)* the form would be marked ***NA/USexcl.,*** meaning that it is exclusively used in the United States. If a form has sporadic use in a number of ***NAs,*** it would be marked, say, ***X NA/US, AU, NZ*** meaning that it is used in the USA, in Australia, and in New Zealand, but not in the other ***NAs.***

7. Indexing for Dialect Areas within the National Areas

National Areas are not homogeneous. Thus in the USA alone we have (minimally) the Eastern Seaboard, the Mid-Atlantic States, the Middle West, the Western States, the South, and the Northwest. Thus, if a term is restricted, say, to one of these particular ***DAs*** within an ***NA*** (e.g., to the Southern States of the USA, as with *hominy grits)* the form would be marked ***USexc.So.*** All of the other ***NAs*** would be divided into the major ***DAs.***

This indexing by ***NAs*** and ***DAs*** within them can, conceivably, result in a reorganization of the entire ***EDE*** according to these principles. The ***EDE,*** in other words, would allow a user specifically to look for Western Australianisms (the area or the city of Perth) under such a rubric.

8. Indexing by Sociological Stratification within NAs and DAs

Use, frequency and recognition of forms is not merely diversified by geography, but also by the ***Speaker's Sociological Status (SSS).*** Accordingly, the ***EDE*** could list whether a certain form is used/recognized by a certain educational-socio-economic class within a given ***DA*** in an ***NA.*** London, England, for example, is a ***DA*** within an ***NA,*** but this ***DA*** is divided into ***Sociological Areas (SAs)*** according to the ***SSS*** criteria mapped by the field workers. A given form X could, then, be listed as ***SSS Cockney cab driver, DA London, NA England.***

9. Feasability and Format

Given the diversity of parameters that would go into such an ***EDE,*** printing and binding seems extremely unwieldy and disproportionately expensive. The way to produce such a dictionary which, incidentally, would be in constant need of periodical updating with new entries added and entries deleted or down-shifted on the frequency scale, would

be to store the information on computer tape. The entries, accompanied by viva voce pronunciations of the entries thus recorded, could be displayed both in orthographic form and in phonemic transcription on a PLATO video panel. All the user would have to hold in his hands in terms of printed matter would be a moderately sized 150-200 page booklet telling him how to ask questions of the computer. The user could type his request into an IBM terminal, requesting, say, all entries coded 'universal English' which are in some sense synonyms of 'steal'. He would get a vast list of entries containing such forms as *rip off, make off with, abscond with, pilfer,* as well as *to expropriate illegally,* with each one subcoded as to its *SSS*.

As the proto-type of the *EDE* gained in popularity and acceptance by universities, high schools and other language centers, further refinements could be added such as *range of collocation, stylistic range,* etc.

10. Conclusions

Proposals that sound utopistic at a given time may suddenly turn out to be quite realistic in fact. The facilities to produce such a dictionary actually exist. With the printing and binding problem out of the way, the international dissemination of the master reels could be carried out at much less expense than one would imagine at present.

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SOCIOLEXICOGRAPHY - A PROPOSAL FOR A REAL DICTIONARY

Laurence Urdang
Essex, Connecticut

Lexicography, it is sad to relate, appears to be falling victim to reactionary principles. It is, indeed, a pity, for lexicography remains one of the last few links between the linguist's world of Akademia and the real world. What other lines of communication are open? Foreign language teaching has, for all proper purposes, disappeared from almost all curricula; comment on usage and other practical aspects of language has passed, for the most part, to the nonprofessional, and we are seeing books by lawyers on British and American English, by TV newscasters on grammar and syntax and style in English, by consumerists on language in the law, by businessmen on "effective" English. Ivory-tower linguists continue to communicate chiefly among themselves and with the few students left to continue the propagation of the species. The Dictionary, it seems to me, is one of the last bastions of defense that will survive the onslaughts of an antitheoretical, propractical, antihumanitarian civilization.

With a blossoming among linguists of interest in meaning, dictionaries have taken on a new prominence. But I am concerned here with a more basic consideration, a more preliminary matter than meaning and definition, namely, entry selection. If a dictionary is to function as a repository of the lexical stock of the language, by what criteria should its entries be selected? First, we must assume that it is impractical and unrealistic to talk about an "unabridged" dictionary - except, perhaps, in the rather private, denotative sense, 'not abridged from a larger source,' which has always been Merriam's justification for that curious usage. It follows that if we cannot produce a dictionary in which all the words of the language are contained, then we must content ourselves with some reasonable criteria for the selection of the entries.

Aside from subjective impressions on the part of the lexicographer, some reflex of frequency would appear to be the sole criterion. Please understand that I have nothing whatsoever against subjective impression by a lexicographer with a catholic background and broad interests, but his subjectivity and personal judgment are best brought into play in the tempering and interpretation of hard data and information. The serious shortcomings of frequency as a criterion begin to appear when one examines the sparsity of resources - really, not more than a handful of investigators have produced anything of much value, and most of that has been restricted to studies conducted of special groups of people, to special fields, or both. Besides, studies like the Thorndike-Lorge one are quite out of date, are not sufficiently broad-based to be meaningful for any but the most frequent words of the language, and represent a sampling far too small to be useful. That study sampled about 7 million words of text; my contention is that valid data for a substantial headword list for a dictionary the size of the average college dictionary would have to be based on billions of words of text. There are two aspects of lexicographic research that, in themselves, prove fruitless, yet they serve as extremely useful adjuncts in the preparation of dictionaries.

One of these is citation files. However, although citation files are useful and interesting in calling attention to new words and senses in the language, they provide no genuine frequency information, and their function must be tempered by the experience of the lexicographer.

The other aspect is what I shall term "category-filling," for lack of a fancier name. Category-filling describes the technique of including, say, all 105 chemical elements just because hydrogen and oxygen and iodine and chlorine and gold, silver, platinum, plutonium, zinc, iron, copper, tin, and a couple of dozen others have been included. Yet, on the basis of frequency alone, lawrencium, lanthanum, astatine, indium, neodymium, praseodymium, and a dozen others would never make it into dictionaries. By the same token, subjectivity demands the inclusion of Pompeii and Surtsey, for they cannot be added solely on the basis of population, which might well be the criterion for including a vast number of other place-names.

It ill behooves me to condemn these approaches; I am merely calling attention to their shortcomings.

The several sets of criteria brought into play in the compilation of a specialized dictionary are often somewhat more complex. For here the lexicographer must deal with encyclopedic and philosophically descriptive entries. Moreover, for many fields within the social sciences in particular, lexicon may be a matter of terminology in which schools will differ in their interpretations, each providing its own set of private terms used to describe phenomena viewed from different points of view. It is the duty of the lexicographer to document such points of view if he is properly documenting the word types of a given discipline. And, lest we are deluded into thinking of a field like chemistry as bound, we should remind ourselves that the names of organic compounds are so profuse as to be virtually undocumentable. The same is true of pharmacology, which is, after all, just a branch of chemistry.

Upon examining, albeit cursorily here, these various approaches to lexicography, we can readily imagine what might not otherwise have been obvious: the paucity of information available on the lexicon of the language mitigates the effectiveness of the lexicographer in producing a work that is in any way scientifically or theoretically viable, notwithstanding its relative usefulness to the casual user who may consult it.

Meager though frequency information may be, it is unscientific to compound the felony by misapplication of what little we do know, and I shall here take to task those lexicographers who do exactly that. But first, allow me to interject a comment:

When I began my study of linguistics in the late 1940's, one precept was pounded into my head, namely, that language is essentially spoken and that written language is merely a stylized reflection of the "real" language - a sort of para- or metalanguage. I don't happen to believe that, for a great many reasons not germane to this talk. But it is interesting to note that for many years the only source, and today certainly the overwhelming source of lexicographic information is from written materials. If we accept the tenet that language is essentially spoken, then by what strange manipulation are proper nouns and adjectives excluded from the main bodies of dictionaries? I was not aware that one could hear a capital letter. While it may be true that a proper name can be identified by syntax and grammar alone, those in-

stances are quite rare and by no means definitive. The problems begin to emerge as one must contend with proper names becoming generics - thermos, lynch, boycott, gerrymander, cellophane, etc. - and with their use in common expressions like Philadelphia lawyer, Boston baked beans, Chicago style [piano], and Dixieland [jazz]. Such analysis also serves to complicate the treatment of proper nouns and adjectives like New Yorker, Glaswegian, and Mancunian. Furthermore, such dictionaries must make the distinction between real people and places (like Macbeth and Valhalla, New York) and fictitious ones (like Hamlet and Valhalla, the Scandinavian heaven). Pity the poor user, who must know so much before he can find an entry in the appropriate part of the dictionary. Pity the poor lexicographer, who must decide whether Homer was real or fictional.

If we examine the average dictionary, we find words like encaustic, ornithopod, trapunto, haustorium, and monazite. Approaching my point gradually, let me emphasize my belief - and yours, too, I trust - that a word like New York, Washington, or (here we go) Coca-Cola is far more frequent than any of those others. I object not to the inclusion of encaustic, ornithopod, and so on in the dictionary but to the warped rationale of frequency that leaves out others of much higher frequency.

The words I am talking about share with "ordinary" words all of the attributes of common language lexicon: they can be spelled, pronounced, defined denotatively, and (usually) etymologized. Moreover, in many cases they are much more highly connotative than "ordinary" words and are often used metaphorically. Surely, they cannot be banished from documentation in a healthy lexicon of English on the flimsy grounds that they are spelled with capital letters!

Perhaps even more than many highly denotative words, these terms reflect a greater degree of interplay between denotation and experience. And isn't that what language is all about?

Because of limited time, we can only glance, and only briefly, at some examples, which I shall present to you in a moment. While listening, bear in mind that although you may feel that your own, personal experience may not correlate with everyone else's when analyzing these terms, the same must be said of the connotations we bring to all words we hear and use. Thus, mother will carry quite different connotations for the mother, for the pregnant woman, for the unmarried pregnant woman, for the pregnant woman bearing her twelfth child, for the child in a family of two children, for the child in a family of ten, for the homosexual, for the orphan, for the person whose mother died last Friday, or is chronically ill, or very old, or who married a man half her age, or twice her age, or is divorced, or widowed, or beats her children, or drinks to excess, or takes drugs, or is rich or poor, warm and loving or cool and hateful, tall and meek or short and tyrannical. . . . You get the idea. But none of these connotative attributes prevents the lexicographer from listing mother in his dictionary, even when he may consider it only half a word.

The fact that not all of the examples following may not be totally familiar to you should not detract from the validity of the exercise: you cannot be - for cultural or other reasons - intimately familiar with every word in the language anyway. What I want you to do is to concentrate, as much as you are able, and to become aware of the reaction you feel to each item in each class individually. After the first few, I shall not be able to pause long enough between items due

to lack of time. I doubt that denotation will prove to be a problem; I should like you to concentrate on connotation, for I believe that therein lies one of the main criteria for the inclusion of words in a dictionary. Note, if you will, the "shock" you experience as you read these extremely common words, which I have grouped into sets in order to give them some cohesiveness as well as better coherence.

1. Daddy Warbucks, Little Orphan Annie, Punjab, The Asp
2. Macy*s, Gimbels, Bloomingdale's, Saks Fifth Avenue, Klein's, Nieman Marcus, Sears Roebuck, Montgomery Ward, Abercrombie & Fitch, Brooks Brothers
3. Hushpuppies, Wedgies, Keds, TopSiders, Buster Browns
4. Ritz Brothers, Marx Brothers, Three Stooges, Abbott & Costello
5. The New Yorker, Atlantic Monthly, Harper's, Saturday Review of Literature, Vogue, Harper's Bazaar, Good Housekeeping, Playboy, Playgirl, Saturday Evening Post, Reader's Digest
6. Anacin, Bayer aspirin, Alka-Seltzer, Bromo-Seltzer, Sedlitz Powder
7. Lucky Strike, Camel, Old Gold, Kent, Kool [Should these have an -s at the end?]
8. Chevvy, Chevrolet, Rolls Royce, Cadillac, Packard, LaSalle, Volkswagen, Maserati, Model T, Porsche
9. Johnny Carson, Merv Griffin, Jack Parr, Dick Cavett
10. Gunsmoke, All in the Family, Hawaii Five-O, The Mary Tyler Moore Show, The Show of Shows, The Waltons, The Ed Sullivan Show, The $25,000 Pyramid, Truth or Consequences, What's My Line?
11. Glenn Miller, Tommy Dorsey, Vaughan Monroe, Glen Gray, Phil Spitalny, Lawrence Welk, big band
12. The Beatles, The Grateful Dead, The Rolling Stones
13. Blues in the Night, Dancing in the Dark, Begin the Beguine, Red Sails in the Sunset, The Music Goes Round and Round, Flat Foot Floogie; Nature Boy; Praise the Lord and Pass the Ammunition, Over There!, There'll Be Bluebirds Over the White Cliffs of Dover, When Johnny Comes Marching Home Again, The Green Berets; Yellow Polka-dot Bikini, Downtown, Mairzy Doats, High Hopes; Back in the USSR, Rock Around the Clock, Lucy in the Sky With Diamonds
14. Chivas Regal, Yago Sangria, Smirnoff, Wolfschmidt, Gordon; Pilsener (Urquell), Ballantine's, Budweiser, Michelob, Coors; martini, manhattan, pink lady, sloe gin fizz, bathtub gin
15. Pan Am, TWA, Air France, BOAC; Concorde, The Spirit of St. Louis
16. The Encyclopaedia Britannica, The Oxford English Dictionary
17. Robert Taylor, Elizabeth Taylor, Richard Burton, Jean Harlow, Sean Connery, Jean Harlow, Candice Bergman, Ingrid Bergman, C. Aubrey Smith, Humphrey Bogart, W.C. Fields, Mae West, Peter O'Toole, Dame May Witty, Edna May Oliver, Joanne Woodward, Mickey Rooney, Shirley Temple, Eric von Stroheim, Charlie Chaplin
18. Tab, Coke, Pepsi, No-Cal, Dad's Root Beer
19. Drano, Liquid Plumber

20. Xaviera Hollander, Polly Adler
21. Toastmaster, Proctor Silex, General Electric
22. General Motors, Exxon, General Electric, Boeing, Ford, Xerox, Texaco, U. S. Gypsum, Eastman Kodak, IBM
23. Crest, Pepsodent, Ipana
24. Columbia, NYU, Chicago, Harvard, Yale, Princeton; Vassar, Smith, Mt. Holyoke; Bennington, Bard
25. Godfather, Godfather II, The Towering Inferno, Jaws, La Grande Illusion, Casablanca, Birth of a Nation, Mrs. Miniver, Deep Throat
26. Leica, Instamatic, Rolleiflex
27. Empire State Building, Hancock Tower, World Trade Center

. . . and many more - by the thousands.

These are the types and tokens of the living English language. Don't you agree that it's high time they were documented? I am eager to learn your reactions, for we are in the process of doing just that.

II

PHONETICS AND PHONOLOGY

A NEW POINT D'APPUI FOR PHONOLOGY[1]

C. F. Hockett

Cornell University

0. Introduction. From its beginnings, the great curse of phonemic theory has been arbitrariness. Many a report on the sound system of a particular language has been accurate enough in the facts covered, yet rendered fuzzy by the reporter's apparently whimsical choice of a particular way of registering and organizing those facts. Attempts to correct this by agreeing on general criteria to be applied in all cases have fallen down because the criteria themselves have turned out to be arbitrary.

To be sure, there are matters that ought to be settled on the basis of convenience, or by tossing a coin; thus, either kilometers or miles are perfectly satisfactory for the measurement of geographical distances. But in phonology it is not at all clear which issues allow that sort of handling and which require more careful pondering.

The aim of this paper is to present and illustrate an empirical point of departure for phonology that may serve to eliminate some of the arbitrariness in our theories and methods. First it is necessary to set the stage. Sections 1-3 do that. The reason this stage-setting is required is not that the new point of departure is difficult or obscure--it is, in fact, very simple. The reason is that phonological theory is currently in such a confused state. We have explicitly to excise and discard various approaches and opinions that are still alive, or have been newly revived, though they should have been permanently abandoned decades ago.

1. Segmentation, Decomposition, and Hierarchical Organization. In the early days of phonemic theory (Baudouin de Courtenay, Saussure, Sweet, Jones, Palmer, Trubetzkoy, Jakobson, Sapir, Bloomfield, and others), the major emphasis was on "letter-sized" units of a kind subsequently known (west of the Atlantic) as segmental or linear phonemes. Yet almost from the beginning it was realized that it is awkward, and perhaps pointless, to try to provide for *everything* distinctive in speech via such units. Even Bloomfield, perhaps the strictest "segmentalist" we have had, extracted stress in languages like English, tone in languages like Chinese. What is more, he accepted (1933) a practice that regarded the syllabic sonorants of

[1]First written 1967. I spoke of implicit motor-matching (§2) at a conference Jan.-Feb. 1967; the report in Neurosciences Research Program *Bulletin* 7:5.465-7 (Nov. 1969) is fairly accurate except for the footnote p. 466. The new point d'appui was outlined to the Cornell Linguistics Club later in 1967, with a repeat about five years later.

bottom, *buttoning* (vs. *lightning*), *stirring* (vs. *string*), *coral* (vs. *Carl*) as consisting of the consonants [m, n, r, l] plus a syllabicity feature that he represented by a vertical tick directly below the symbol for the consonant.

I think Bloomfield would have defended this treatment of syllabic sonorants only on the grounds of notational convenience. Any deeper reason would seem equally to justify steps he refused to take, or which he viewed as unmotivated: for instance, interpreting English [m, n] as [b, d] plus a feature of nasalization. Perhaps he would have argued on grounds other than pure expediency for the extraction of English stress or Chinese tone. But in this connection we must remember that the issue of segmentation versus decomposition was obscured in earlier phonemic theory by two sources of confusion: an uncertainty as between "word phonology" and "utterance phonology," and a failure to distinguish sharply between phonology (or phonemics) and morphophonemics. Only in the 1940s did we begin to straighten these matters out so that the problem of segmentation versus decomposition could be attacked relatively free of irrelevancies.

Let us be clear on these two issues before we proceed.

Probably everyone would agree that, in principle, phonological analysis must acknowledge all those features of articulation or sound that are distinctive for whole utterances. The older way to do this took for granted the reality and relevance, in any language, of units called "words." The "word phonology" of the language was then provided for by tabulating the phonetic units that serve to distinguish words from one another, and the arrangements in which those units occur within words. That left for separate handling those features that appear only when words are used as whole utterances or are put together into utterances. Often this second chapter of phonology was left blank, but that was a defect of practice, not of principle. The real source of inadequacy in this older approach is the indefinite and variable status of the units called "words." Even in treatments of a single language this term is not used unequivocally. In general, the recognition of wordlike units (by either analysts or users of a language) involves resort to semantic criteria as well as to phonetics. The extent to which word identity is maintained phonetically in running discourse then varies greatly from one language to another; this variability is one of the facts that the older approach via "word phonology" tended to obscure.

We sacrifice no information, and give up no access to relevant information about a language, if we ignore "words" altogether in phonological analysis, insisting instead on whole utterances as our units of observation and comparison. But that puts the matter negatively. Empirically, we *must* base phonological analysis on whole utterances. It is whole utterances, not words, that people utter; it is whole utterances, not words, that people hear and respond to; it is whole utterances, not words, to which a child is exposed as he learns his native language. It does not at all matter that some short utterances contain only single words--that is a fact discovered, by analyst, user, or learner, only through exegesis of a sort that goes be-

yond phonology.

Word identity, then, has nothing to do with phonology. No more does morpheme identity. This eliminates at one stroke the earlier confusion between phonemics and morphophonemics. That a particular morpheme or a particular word appears in such-and-such a phonetic shape, or that it appears now in this shape, now in that, is morphophonemics. That the shapes in question are *available* for such use is phonemics. Phonemics, or phonology, deals only with the economy of the phonetic material used in a particular language, not with how it is used. Of course, the latter is vitally important. But it is not phonemics.

The conclusions just set forth were reached in the 1940s--but only by some investigators; others ignored, misunderstood, or rejected them, and many still do. One apparent difficulty was cleared up only by the mid 1950s. It might seem that "utterance phonology" implies that even a very long utterance is composed, phonologically, of a mere linear succession of phonemelike building blocks and nothing else. Of course that is not true. The phonological structure of an utterance is *hierarchical*. The patterns of organization differ from language to language, but we can illustrate with English.

Unless it is interrupted by false starts, hesitations, or other such paralinguistic phenomena, an English utterance consists of a series of one or more *macrosegments*. Each macrosegment bears an *intonation* and is marked off by certain intonational phenomena from what precedes or follows. Apart from its intonation, a macrosegment consists of a series of one or more *microsegments*. The intonation places *primary stress* on exactly one of the microsegments of the macrosegment. Successive microsegments within a single macrosegment are separated by occurrences of *juncture*.

An occasional microsegment, in context, consists of isolated consonantal phones: *He oh'd and ah'd* (= 'He said "oh" and "ah"') has two occurrences of isolated [d]. Most, however, consist of a series of one or more *syllables*. Each syllable includes a *peak*. Whatever precedes the first peak of a microsegment is an *onset*; whatever follows the last peak is a *coda*; and whatever intervenes between two successive peaks of a single microsegment is an *interlude*. In general, the number of syllables in a microsegment can be counted (a few cases are tricky), but when this is true it is because of the countability of their peaks. Syllable boundaries are phonetically indefinite and structurally irrelevant except when they coincide with junctures between successive microsegments or with the boundaries of a macrosegment.

At most one peak of a microsegment carries *loud stress*. If the intonation of the macrosegment places primary stress on the microsegment, it is usually on this peak that the primary stress falls. Elsewhere within a macrosegment, loud stress is *secondary stress*. Peaks within a microsegment that do not carry loud stress carry *tertiary* and *quaternary* stresses. Thus, within a single microsegment there are at most three contrasting degrees of stress.

Apart from stress, a peak is *simple* or *complex*. A simple peak

consists of a single phonetically homogeneous phone, usually of the type describable as "vocoid" (some might want to withhold that epithet from my retroflex peak in *skirt*, *Berber* and the like). A complex peak contains a diphthong with first part more prominent than second part.

There are zero onsets (*aloft*, *often*), zero codas (*coda*, *bitter*), and zero interludes (*paying*, *owing*). Otherwise, an onset, coda, or interlude consists of a combination of one or more phones of the kind usually described as "consonantal."

Microsegments in English are not the same as words. In *I'd like to*, the contraction *I'd*, like most or all such contractions, is one microsegment but two words; *slyness* and *Plato*, each surely a single word, are often pronounced as two microsegments each. However, a great many English words--probably most of the everyday vocabulary--occur always or usually as single microsegments. This is a morphophonemic fact that cannot even be discovered unless one has taken the whole-utterance approach in phonology; but it explains why, for English and for languages that resemble English in this particular respect, the old approach of word phonology was able to discover so much of genuine relevance.

Some languages are quite different. Fox, for example, seems to have no juncture and no microsegments: the macrosegment consists directly of syllables, with peaks, onsets, and interludes, but no codas. Accordingly (and assuming that my observations are accurate), in running discourse word identity is not maintained even in the partial way it is in English, but is altogether lost.

Note that the fact that words are not phonological units does not in any way prevent us from speaking freely of "the pronunciation of a word." Some words in some languages are pronounced in various different ways, depending on environment. But normally when we refer to the pronunciation of a word we mean the phonological structure of an utterance which contains only that one word, minus any phonological features that belong, as it were, to the whole utterance, or to the single macrosegment in it. Thus, when describing, say, the English word *halt*, we must specify consonants and a vowel, and loud stress, but we need say nothing about the intonation with which the word is uttered on one or another occasion, and we need say nothing of juncture, because there is no juncture *within* the form when it occurs as a whole utterance.

Now we can return to the issue of segmentation versus decomposition.

Although in the 1940s the issues we have just reviewed were not yet clear, it is essentially correct to assert that the problem of segmenting or decomposing presented itself at that time in the context of phonological hunks of approximately the level of complexity of English microsegments. In the early 1940s in the United States, perhaps largely through the influence of Trubetzkoy and his colleagues of the Prague school, some of us started to wonder whether the setting-up of "letter-sized" units (as the units of which microsegments or the like are composed) can have *any* justification other than that of notational

convenience. Action on this suspicion was delayed because so many of us were caught up in a fascinating game we had invented: a game whose moves were the assignments of (segmental) allophones to (segmental) phonemes, and whose rules were such criteria as "phonetic similarity," "complementation," "pattern congruity," and "simplicity"--the last often interpreted merely as "smallest possible number of phonemes." As an illustration of this game we need consider only my own paper of 1942: through §5 of that paper the discussion is reasonably empirical, but with §6 the arbitrary game-playing begins. Yet, despite this source of delay, there were tentative experiments with phonological decomposition, which in one form or another has now become widespread.

Unfortunately, thoroughgoing decomposition meets snags just as does thoroughgoing segmentalization. When a language presents us with a neat set of consonants, say with

[p	t	k
b	d	g
m	n	ŋ],

then the way is seemingly--perhaps deceptively--clear. As components we are tempted to recognize bilabial versus apicodental versus dorsovelar, voicing versus voicelessness, and oral versus nasal. But the sounds of a language do not necessarily all come in such neat sets. What do we do with an odd [l] or [r]? What do we do with the consonant system of Plains Cree,

[p	t	c		k	
		s			h
m	n				
w			y],		

where the only discernible symmetric set is that involving [p, t, m, n], and where any articulatorily realistic decomposition seems to yield as large a stock of ultimate components as there are consonantal segments to begin with?

Furthermore, *what* do we decompose? Do we first play the traditional game of linear phonemicization, and then decompose the phonemes? (Prague practice was something like this.) Or do we bypass the game and decompose phones? If the latter, then how do we provide for limitations on distribution and cooccurrence? These last are in our data, not in our approach: our methods must reveal them, not conceal or distort them. For all its arbitrariness, the linear game as played in North America a third of a century ago yielded one important byproduct: though perhaps for the wrong reasons, it forced a scrutiny of distributional facts far exceeding in depth of detail that customary anywhere else or since. It would be a shame to lose that part of our heritage.

2. Articulatory or Acoustic. Another problem is whether decomposition, or any other procedure in phonological analysis, should rest on articulation or on acoustics, or perhaps in some way on both.

In order to get at this issue realistically, we must ask still

another question--one which, phrased in one way or another, lies at the very heart of phonological theory. We shall phrase it this way: When we hear someone say something in a language we know, how do we know what is being said?

Of course, sometimes we *don't* know. That is important, and we shall return to it later. First, however, let us consider the cases in which we do.

Now, it is undeniable that speech produces sound, and the usual intelligibility of conversations between people who cannot see, touch, or smell each other--as, over the telephone--means that the speech signal normally carries enough stigmata for the hearer to know what has been said. Thus it is entirely reasonable to propose that it is the sound of speech that counts; therefore, that phonemic analysis should rest on acoustics, and should, in fact, consist in identifying, and tabulating the distributions of, the acoustic stigmata that keep utterances apart.

This was the assumption of Trubetzkoy and Jakobson in the heyday of the Prague school. Their phonetic terminology purported to rest on acoustics. The motions of articulation were relegated to secondary status: they are merely the mechanical means by which a speaker introduces the appropriate acoustic features into the speech signal.

This view, like the linear phonemics game of American linguists, had one important positive result. There is no merit in considering the possible motions and positions of the articulatory apparatus merely as motions and positions. They are not likely to be linguistically relevant except insofar as they yield sounds that the human ear can learn to differentiate. The bilateral symmetry of the body, including the speech tract, means that we need make no provision for a possible distinctive use of a left-sided articulatory motion versus a right-sided one, although kinesthetically such motions are easily distinguished. Again: it is well known that there are two quite different tongue-positions that can be made to yield auditorily indistinguishable "r-color." Still again: Jakobson taught us that a [p] and a [k] have this in common, as over against a [t]: that the closure for either of the first two leaves the oral cavity to function as a single undivided resonance chamber, whereas the closure for [t] divides it into a larger one behind the tongue and a smaller one in front. This helps us to understand the not uncommon interchange of labials and dorsovelars, as in English *He bumped/bunked into me*, *Peep/peak around the corner*, or current Dutch [xt] for earlier [ft].

The acoustic bias of the Prague phonologists was developed before the invention of the sound spectrograph. With the rise of sound spectrography in the mid 1940s, a strange thing happened. Proponents of the Prague approach were able to produce spectrograms on which they believed they could precisely identify the traces corresponding to the various acoustic features already identified and labeled in their tradition ("acute," "grave," "compact," "diffuse," and so on; see Jakobson, Fant, and Halle 1952; Jakobson and Halle 1956). Others were unable to match these results, and were critical of them--at times quite

sharply so (Joos 1948 p 8 fn 8; Joos 1957).

Particularly important here is the research in acoustic phonetics carried on in the 1950s and 1960s at the Haskins Laboratories in New York by Delattre, Cooper, Libermann, and various colleagues. The upshot, supported by comparable studies at various other centers, is this. A spectrograph can be adjusted to match the response characteristics of the human ear. When this is done and spectrograms are made of natural speech (of the sort that is perfectly intelligible when heard), no one can read them. Furthermore, no one can *learn* to read them. The stigmata labeled "grave," "acute," and the like by the Prague school are not discernible to the eye. This does not mean, of course, that the speech signal is devoid of stigmata--it could not mean that, since, after all, we talk with one another. It does mean that the Prague doctrine was an almost unbelievably great oversimplification of the facts. The strategy of acoustic phonetic research, it appears, must be as follows: one examines spectrograms of speech whose phonological structure is *independently* known, so that the acoustic correlates of the phonologically relevant units can be sought. They can be found, but they are enormously varied, and the correlations are exceedingly complex.

The unreadability of spectrograms is not due to any defect in the spectrograms. They contain all the information necessary for the understanding of speech--but coded in the wrong form for human consumption. If a spectrograph of intelligible natural speech is put on a good pattern-playback and mapped back into sound, it comes out again as intelligible speech. But seeing and hearing are different arts. After all, one can feed a television signal to a loudspeaker (with an appropriate stepdown of frequency) and thus render it audible, but that does not make it possible for anyone to "see" with his ears. Why should we be able to "hear" with our eyes?

Since speech communication makes use of sound, we must pay attention to how human beings receive and interpret sounds. If I am called upon to describe something I have heard from the next room, I will say "it sounded like someone scratching a mirror with his fingernail" or "like someone striking a bookmatch" or "like a heavy book sliding off a table and falling to the floor." I may characterize a strain of orchestral music by saying "the melody was played by flute and English horn in octaves." Sometimes I may use vaguely onomatopoetic words: *thud, thwack, screech, crunkle*--onomatopoetic *English* words, be it noted, by no means automatically intelligible to those who speak only other languages. Occasionally I will use certain simple terms of direct acoustic reference: "a *high-pitched* squeal," "a *loud* bang"; but there are very few words of this sort and we are rarely able to rely on them wholly.

In all of this I am surely typical not only of my own culture but also, in a general way, of our whole species. Our usual way of describing sounds, of classifying them, and of discovering how to react to them appropriately, is in terms of *the actions or events that produced or may have produced them*. Until we learn better, we handle a new sound of unknown source in terms of its similarity to familiar old

sounds of familiar origin. The ear, true enough, responds to frequency, to amplitude, and to their changes in time, within certain fairly well understood bounds. Hearing, however, is much more than that: it is a performance not alone of the ear but of the whole body and its total accumulated experience. This complicated whole-body skill is called into play when suitable information arrives via the ears; it cannot manifest itself when exactly the same information--"same" in some abstract sense--arrives via some other sensory modality.

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I want to diverge from the main argument briefly, to underscore the whole-body nature of the art of hearing by considering what happens when a musical person listens to music or a poetic person to poetry.

It is not possible to make sense in discussing music unless one distinguishes between *meter* and *rhythm* (distinct from both is *tempo*, the absolute rate of the meter; but it will not enter into the present discussion). When a musical person listens to a piece of music, he seeks rapid orientation as to its underlying metric (not rhythmic) pattern. He may have it before the performance begins if the program announces the piece as a waltz or as a march, or if he has the score in his hands and sees the time signature, or, of course, if he has heard the same piece before. Otherwise, the first few notes he hears serve to establish the meter; if, later, there is a change, the first measure or so after the change establishes the new pattern. Once the metric pattern is conveyed to the hearer, it is maintained, not necessarily in the music, but in *him*. In one way or another he beats time--perhaps with a toe, or with a leg draped over the knee of the other leg, or with slight motions of head or hand; perhaps in several of these ways; sometimes entirely in the central nervous system (the last of these is recommended when the listener is part of a concert-hall audience). The *rhythm* of the music is then the temporal distribution of its more and less prominent notes relative to the underlying meter. All sorts of distributions are possible. In, say, a basically duple-time piece, there can be a passage in which not a single prominent note coincides with the strongest metric beat. The total effect is sometimes confusing--many people find Brahms's rhythms too complicated for enjoyment. Of course, there are people in our culture who have not learned this important responsibility of him who would enjoy music: they are the totally unmusical ones, the ones who, in ballroom dancing, keep a time (if any) totally unrelated to the meter supplied by the band.

Very much the same is involved when a poetic person listens to poetry (or reads it, aloud or silently, to himself). In poetry, as in music, meter and rhythm must be distinguished. It is the poet's responsibility to convey information quickly--even in advance, as by typography or title--about the meter; it is then the recipient's responsibility to keep track of the meter, so that he can register the counterpoint between the underlying meter and the actual rhythm of the words. It is not surprising that shorter poems are often read at

least twice: the first reading is a sort of rehearsal. Students of English prosody, particularly those with some linguistic training (for example, Epstein and Hawkes 1959), seem largely to have overlooked the point being made here. The structure of an English poem is not just its phonological structure as an English discourse. Something has been added. The added ingredient is the *distinction* between meter and rhythm, which can be properly maintained only through the skilled participation of the audience.

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We cope with sounds, then, in terms of their sources. This is a general characterization; it applies to the sounds of speech as well as to sounds of all other sorts.

Beyond that, the art of hearing speech seems to involve a factor that plays no part in some other kinds of hearing. In general, our recognition of a sound does not mean that we could produce the same sound ourselves. I recognize the sound of a flute, which I play, but I also recognize the sound of an English horn, which I cannot play, and the sound of thunder, which nobody plays (except Thor). But a speech signal in a language we know is produced by articulatory motions in which we are thoroughly schooled. The most obvious and realistic theory of speech reception is that we identify what is being said by a sort of implicit matching process: by discovering, through trial and error, what articulatory motions we would ourselves make, under similar noise conditions, to produce the same acoustic effect. Sometimes, when we have encountered unusual difficulty in understanding because of ambient noise or poor enunciation--imagine leaning over a dying man to catch his last words--we are aware of this motor-matching process; we may even articulate aloud, instead of confining the activity entirely to the central nervous system. Usually, however, the process goes on entirely implicitly, and entirely out of awareness --the latter in just the same way, and for just the same reason, that other skilled tasks, such as the fine muscular adjustments of steering a car or operating a typewriter, become automatisms that we can perform without conscious effort.[2]

It would be pleasant if one could say at this point, in a spirit of tolerant compromise, that phonological analysis can equally well be based on either articulation or acoustics. This is what some of us have said occasionally in the past. But the evidence, as I see it, really dictates otherwise. Despite the marginal evidence from pathology, and despite the fact that careful research in acoustic phonetics does discover acoustic correlates of linguistically relevant units,

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[2]There are occasional individuals mute from birth who nevertheless learn to respond more or less appropriately to the speech of others. For them, motor matching is impossible. Therefore we dare not push the theory to the extreme of claiming that speech can be understood *only* by virtue of motor matching. But I don't think one should argue too strongly from the pathological situation, which, through most of human history, has been contrasurvival.

the emphasis of the Prague school must be inverted. The speech signal is of no importance in itself. It is important only in that it is always the principal, and sometimes the only, carrier of information to the hearer as to the targets towards which the speaker has aimed his articulatory motions. It is those targets of articulatory motion that are of prime importance.

We should remember that the hearer's evidence as to what the speaker is saying very often does not consist *exclusively* of the speech signal. In face-to-face transactions, the receiver can see certain portions of the speaker's articulatory motions. In silent mouthing, which is sometimes intelligible, these visible parts of articulatory motion are exaggerated, and no speech signal is produced. In any case, speech is accompanied by all sorts of body motions, which in any one community are conventionalized to correlate in certain ways with what is being said. In English, any speaker kinesically underscores his louder stresses with slight motions of eye, head, or hand: this can render a spoken message intelligible to a hearer even if the speech signal at the particular moment has been mangled beyond recognition by ambient noise. We are largely unaware of these accompanying visual signals, but we are trained from infancy to register them. They sometimes even accompany the silent "internal speech" of someone who is "thinking in words"; and so, occasionally, under unusually favorable circumstances, an observer makes a good guess at what someone is "thinking about," usually to the surprise of the latter, and sometimes convincing one or both of the existence of telepathy. Also, the speech signal itself, plus the visible part of articulatory motion (if the speaker is in sight), carries more than just linguistic information. It carries all the paralinguistic accompaniments of and insertions into speech, and these, like kinesic signals, help a hearer to understand what is being said.

Our theory needs no modification to provide for the obvious fact that people who speak divergent dialects of a single language can often understand each other. But in accounting for this fact we do need to recall a well-known point that has not yet been mentioned: that every phonological system is redundant. What this means is simply that not all combinatorially possible (or even all physiologically possible) arrangements of the phonological ingredients of a language constitute utterances in that language; in fact, only a small percentage do. The connection with hierarchical patterning is close. If all arrangements were possible, there would be no such patterning. In fact, however, if (by way of an example, in English) I have begun a microsegment with [sk], I can build a more complex onset only by adding [w, y, r], or [l], after which--or without which--I am forced to continue with the very different kind of phonological material that forms English syllable peaks. Such facts as this hold whether I am saying something or only spouting nonsense--provided the latter is *English* nonsense.

Orthographic systems are redundant just as are phonological systems, and since redundancy is at bottom the same sort of thing in either speech or writing we can perhaps validly illustrate in terms of

writing. Below I give five strings of capital letters (from an alphabet of twenty-seven letters, counting space as one). None of these strings is an actual written sentence in any language known to me. All were constructed in a partly random way, but with certain statistical constraints. In each string, the constraints were some of those (not, of course, all) of a particular written language of Europe. I believe the reader will have no difficulty in identifying the language in at least a majority of the cases:[3]

1. DIAL AMERANTEN DE LA CAL CULA ROSCA UNDE QUIVERDIRSE
2. EBEITEN KON DAGEN EINRELATE EIE EI MOGISTEN AUFEN WEI SONKLABIATZ ZWISLAUF UNDIEDE OPHOBESE SO STATZ UNDER DIES VONUR DALBEN
3. CZONCO WY CZESZEDSZKAWSP BALNIKA O POSOBSZAROKLANCIOWIAJUGO
4. IN NO IST LAT WHEY CRATICT FROURE BIRS GROCID PONDENOME OF DEMOSTURES OF THE REPTAGIN IS REGOACTIONA OF CRE
5. DES GROMOGEN FRE DE POIS LANIE LA PREST CERTEMPROPROBJE TICHIFFRESTENT LE REDIE DE DET A JE

Even when different systems use the same elements--as these five written languages all use the Latin alphabet, and as all the world's spoken languages use articulatory motions of which human beings are capable--each imposes its characteristic statistical structure on the arrangements that actually occur. Of course, that is not all that is involved. Even the whole set of statistical constraints discoverable in any one system would not be enough to guarantee the invariable intelligibility of "messages" whose generation was otherwise random. On the other hand, it is significant that even a small portion of the statistical constraints of a particular system is enough to make nonsense strings resemble messages in that system more than they resemble messages in any other system.

Now let us see the relevance of redundancy. Suppose you and I speak identical dialects, and that you speak to me in the presence of a certain amount of ambient noise; or suppose that you and I speak divergent dialects, and that you speak to me under less noisy conditions. If I correctly identify a sufficiently large portion of the articulatory targets you have aimed at--typically, it would seem, about half will do--I can ordinarily fill in the rest from context. The filling in, however, will conform to *my* articulatory habits, not to yours--even if, in addition, I note a general cast of difference and diagnose it, from previous experience, in terms of your possible provenience. Special circumstances may draw my attention to the fact that you do not distinguish (as I do) between *caught* and *cot*, *bought* and *bot*, *law* and *la*, but most of the time, when my concern is with what you are saying, not with how you are pronouncing things, I will

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[3]One of these is from Shannon (1949); I constructed the others.

hear each word as it would be in my own dialect, quite unaware of the missing contrast. Of course this works the other way too: lacking the contrast in your speech, you are not likely to notice it in mine.

We must carry our theory one step farther; it is not yet quite complete. The phonological structure of an utterance does not consist of the actual articulatory motions of the speaker, nor even (as I may have seemed to imply earlier) of the succession of targets towards which he has more or less carefully aimed those motions. It consists, rather, for any one hearer, of the succession of targets towards which that hearer would aim *if he were producing the same utterance*.

Now, every speaker receives his own speech, both auditorily (by bone and air conduction) and kinesthetically; indeed, he constantly monitors his speech via these feedbacks. Thus, under our definition, any utterance does have a phonological structure for its speaker, not because he is producing it but *because he is receiving it*. Perhaps it is normally this particular phonological structure of the utterance that concerns the linguist. But nothing prevents a single utterance, as a unique historical event, from having other phonological structures for other hearers. In fact, if that were not possible, then no one would ever misunderstand anyone else's speech.

We said at the beginning of §2 that we would return to the fact that we don't always understand one another, and here we are. It behooves us to pay more attention than we usually do to breakdowns in speech communication. We linguists tend pridefully to think of language as an enormously powerful means of communication--and so it is, but only because its users are skilled in ignoring trivial failures and in circumventing the consequences of more serious ones before it is too late. Since, as users of language, we linguists share these skills, it is difficult for us to realize how very frequent such breakdowns are. In actual use, language constantly skirts the thin edge of unintelligibility. My own observations suggest that momentary breakdowns occur dozens of times in the course of an ordinary day's routine.

But it is significant that breakdowns are of two different sorts. Misunderstanding is one thing; failure to understand is another. When I asked a New York City waiter thirty years ago for *orange juice* and he calmly started to bring me *oysters*, it was a case of misunderstanding: that was what he had heard, and that was what he thought I had said. If he had been unable to make sense out of the signals he had received from me, and had asked me to repeat, it would have been a case of failure to understand. Considering the variety of noise conditions under which conversations are held, it is easy to understand why there should be many failures to understand. But how about misunderstandings? Why should I ever hear *Y* when you have in fact said *X*?

In part, we have given the answer in our proposal that the phonological structure of an utterance consists of the succession of articulatory targets towards which the hearer would aim his articulatory motions in order to produce the utterance. We have also mentioned the

crucial factor of redundancy--not only of sound systems, but of the whole communicative package of which a speech signal is only one part. But one further point must be made.

It is clear that a speaker *plans ahead*: at almost any point in what he is saying aloud, he may be silently marshaling various alternative ways of continuing, and choosing from among them. Often enough this silent *covert editing* erupts into audibility and we actually hear words or phrases broken off in favor of some alternative the speaker has meanwhile thought of and has decided is better. This observable phenomenon is *overt* editing, and is an important part of our evidence for the reality of covert editing (the rest of the evidence is hesitation-sounds and elementary introspection). Speaking, thus, is a complicated and active process.

Receiving speech is just as complicated and active, even though silent. Usually though not always out of awareness, the hearer "plans ahead" too, on the basis of what he has heard so far: that is, he assembles the most likely possibilities as to what will come next from the speaker, and is more set to interpret the next evidence that reaches him in terms of those possibilities than in terms of others. His receptive and interpretative apparatus for speech has a statistical *bias*. This operation has been aptly described by the aphorism "we listen for what we expect to hear" (Cowan, 1965). Nor is this all. Sometimes--doubtless much oftener than we realize--a hearer does a "double take," recalling recent gross input from short-term storage and rescanning it in order to interpret it, if necessary, in terms of what actually did follow. Here the following quote is relevant; it applies in principle to almost any speech communication in the presence of slightly more noise than usual (Westlake 1960 p 120):

> The voice had an annoying habit, every once in a while, of talking like a telegram, which meant that Roger had to let the words circle around in his head two or three times, while inserting verbs and pronouns and modifiers, until he figured out what the message had been.

All this is possible because the hierarchical patterning of utterances enables the hearer to listen in terms of *general contours* (*Gestalten*). His running predictions of what will come next are made not after each successive minimal linear unit (phone or "phoneme"), but in terms of considerably larger hunks; only when a quick scan of the most prominent features of an utterance proves inadequate for the satisfactory identification of that utterance does he rescan to detect further criterial detail.

Consider, for example, an experiment once conducted by Y. R. Chao (1968 p 170):

> Once over the telephone I asked Robert W. King . . .: "Pam you ungelfpangg thob I fay?" and he answered promptly: "Yes, I understand you perfectly, but you talk as if you had something in your mouth."

Consider, next, the experience of being in a room where two or

more conversations are going on at the same time. We can focus on one until we catch the drift; then we have relatively little difficulty understanding what is said in that one, while the sound of the other conversations is filtered out as noise. We are predicting (and, as necessary, rescanning) in terms of the conversation we are following. We are listening for what we expect to hear in *that* one, not for what we would expect to hear in the course of any of the others. So far, it has not been possible to train spectrographs to do this.

Consider, finally, the following sort of common experience. A friend brings a stranger into my (presumably) quiet office and says "Chas, I'd like for you to meet Mr. Mrmpff." At least, that is what I make of it. The redundancy of ordinary discourse, of pat phrases and formulas, is enough to combat the noise-level in the room, and until my friend reaches the last word I am quite unaware of any noise at all. With the last word I suddenly realize that the surroundings are noisy indeed. The trouble is that there is so little basis for reasonable prediction at this point. An American surname can be almost anything pronounceable in English--it requires considerable charity to regard some of them as pronounceable at all. Nor is there any clue, in the stranger's appearance, as to whether his name is more apt to be *Smith* or *Prokokofieskovitch*. The predictive process breaks down, and rescanning does not help: I do not misunderstand, but I fail to understand.

Now it is only in very rare circumstances that we can know *exactly* what a speaker is going to say next. There are literally thousands of pat phrases and formulas in every language, and we hear them over and over again; but it is also true that we hear, every day, numerous utterances that are being freshly coined. If in actual historical fact we have heard some of them before, we have forgotten them. The probabilistic economy with which such new utterances are coined by speakers is such that listening for what we expect to hear usually works--since the probabilistic economy of the language, or, for that matter, of the whole culture, in our own heads is reasonably close to that in the head of the speaker. But for this rapid adaptability to the novel we pay a price. The price is the frequency of *mis*understanding (rather than mere failure to understand). I make my reasonable running predictions, and expect that the next phrase I hear will most probably be one of a certain roughly delimited set. What you actually say next happens, however, in terms of my expectations, to come from way out in left field. If I insist on hearing in terms of my expectations, I hear wrong. The point is that we could never hear novel utterances correctly--we could not use language at all--except by virtue of an art of listening to speech that forces us sometimes to hear wrong.

ooooo

My aim in the foregoing lengthy exposition has been to demonstrate in full detail that articulation, rather than acoustics, not only affords a possible basis for phonological analysis but, in fact, constitutes the only empirically correct basis. Surely we are free to speak of the pitch and volume of the glottal tone directly in terms of

pitch and volume; as we have seen, those are elementary acoustic factors for which we have reasonably accurate everyday terms. But when it comes to, say, the difference between an [f] and a [θ], articulatory description is indispensable. Research in acoustic phonetics is a worthy enterprise, but its aims are at most tangential to phonology.

In sum, then, I take the phonological system or *sound system* of a language to be the (finite) stock of articulatory targets towards which a speaker aims his articulatory motions, in certain hierarchical arrangements, and in terms of which a hearer interprets the incoming speech signal in order to determine what is being said.

3. "General Phonetics" as a Frame of Reference. If phonological analysis must rest on articulation, it is important for us to have at hand a thorough, accurate, and viable doctrine of articulatory phonetics, and for us to use it wisely.

In a general way, we are served very well by the tradition that comes to us from Sweet, Passy, and others, through the I.P.A., suitably enlarged by students of "odd" languages in various parts of the non-European world. The phonologist must exercise care that he does not allow the traditional "general" classification of speech sounds, established as a matter of convenience on the basis of a limited number of rather similar languages (chiefly French, German, and British English), to force erroneous interpretations of the data of some new language. Emeneau (1939) would have been utterly baffled by the vowels of Badaga had he assumed, as a poorly trained investigator might have, that when vowels differ in quality the difference can only be a matter of tongue height, tongue advancement, or lip position. These classical three dimensions of vowel quality are inadequate even for some features of my own sort of English, as I shall now demonstrate.

My vowel in *cap*, *cat*, *catch* is low front unrounded; that in *pep*, *pet*, *ketch* is mean mid front unrounded. The diphthong of *yeah* begins with the position for the second of these vowels, then centers and lowers. But the diphthong I have in *ham*, *man*, *candy*, sometimes in *bad*, will not fit. It does not begin with the tongue position for either *cap* or *pet*. The tip of the tongue is where it is for the vowel of *cap*: behind the lower teeth (for *pep* and *yeah* it is slightly higher). A region just behind the tip, though, is bunched up towards the alveolar ridge, approximately where it is for *pep* and *yeah*. The glide is then centering and lowering, as for *yeah*; but the diphthongs of *man* and *yeah* are not articulated the same and do not sound the same. Perhaps this seems trivial; yet in order honestly to describe my pronunciation of *man* I have had to go beyond the classical I.P.A. trinity and distinguish the position of the blade of the tongue from the position of the tip. My vowel in *man* is low front unrounded *laminalized*; that in *cap* is not laminalized. (Similarly, the short front vowels of German *ich*, *echt* are laminalized; the nearest English vowels are not.)

Even when tongue height, tongue advancement, and lip position do seem to suffice, we can be misled by the traditional way of handling these factors. This brings us to my first more extended demonstra-

tion: Fox.

Fox has four short vowels. If we follow our traditional custom of placing symbols for these in a two-dimensional array, those for fronter vowels to the left, those for higher vowels above, we get the following display:

[i o
e a].

(The symbols are chosen for typographical simplicity: reading from left to right and top to bottom, the qualities are respectively about like those of English *bit*, *book*, *bet*, and *but*.) Now let us analyze these four vowels as bundles of articulatory components. We are led to recognize four such components: highness versus lowness, and frontness versus backness. Each vowel, as a bundle, contains one component from each of these pairs.

There are some troubles with this.

The first is that our treatment has completely ignored the fact that Fox [o] regularly involves lip-rounding, while none of the other three vowels does. Honesty demands that we somehow provide for this--perhaps by acknowledging roundedness as a "secondary" or "determined" feature that appears whenever backness and highness cooccur. But that seems arbitrary, in that the selection of certain features to be "basic" and of others to be "determined" appears to rest more on our whim than on anything in the data.

A second trouble is that there seems to be no way of recognizing any of our four components as recurring in any of the consonantal bundles of the language, except in the semivowels [w] and [y]. Perhaps that should not bother us. The I.P.A. tradition proposes that the articulations of consonants and of vowels are largely different, so that in general the features discernible in vowels are not to be looked for in consonants, nor vice versa. Jakobson, as is well known, has taken a different view. I have made it clear that I regard his putative acoustic basis for analysis as an empirical error (§2). But that does not necessarily imply that his opinion on the affinity of vowels and consonants is wrong--perhaps one can find an articulatory basis for it.

About a decade ago (1964-1965), doubtless unconsciously guided by ways of thinking I had learned from Jakobson, I stumbled across something that may handle Fox less arbitrarily. If I make the Fox vowel [i], and then explore with my articulatory organs in search of the Fox stop consonant whose position of articulation is nearest, I find the consonant [č]. For [i] the blade of the tongue is relatively high; all I need do to get to the position for [č] is to raise the blade, behind the tip, until it is in contact with the roof of the mouth along and behind the alveolar ridge. A similar experiment showed that I can get from the position for [e] to that for [t] merely by moving the tip of the tongue slightly forward, into contact with the upper teeth. In the same way, [a] and [k] are paired off, as are [o] and [p]. That accounts for all the short vowels, and also for all the stop consonants Fox has.

For clarity I had better expand on this a bit. When I make the Fox vowel [i] and then explore, I get the kinesthetic impression that the blade of my tongue is closer to the alveolar ridge than any other articulator is to any possible point of articulation. For [a], the minimum distance between the lower and upper margins of the oral cavity is between the dorsal part of the tongue and the velum. The affinities of [e] and [t], and of [o] and [p], are similar. I don't know that direct measurement (as by X ray) would confirm these impressions. Perhaps they would not--perhaps the impressions actually rest on relative muscular tension or something of that sort. But they must rest on something, and my working assumption is that they afford us an articulatory basis for Jakobson's view of the affinity of vowels and consonants.

Let us recognize a common feature, *stopness*, shared by Fox [p, t, č, k], and another, *vocality*, shared by [i, e, a, o]. Then, in addition to these features, [e] and [t] share a feature that we will call *apicality*, [i] and [c] share *laminality*, [a] and [k] *dorsality*, and [o] and [p] *labiality*.

Note that the vowel bundle that contains labiality is the one earlier described as having lip-rounding. It turns out that, from this angle of approach, labiality, instead of being a secondary matter to be noted almost by way of afterthought, is just the crucial basic thing about [o] as over against the other vowels.

Is there any gain? Do we not now, for the sake of honesty, have to add that when vocality and labiality cooccur one also has highness and backness as "determined" or "secondary" features? Well, I am not sure that we do. Certainly, the literature describes Fox [o] as like the vowel of English *book*, and that is how I tended to hear this vowel in the Oklahoma Kickapoo dialect of the language. In fact, however, there is a good deal of variation. The Fox texts of Jones (1907) and of Michelson (e.g., 1921) were neither taken down nor printed in "phonemic" transcription, and they show a number of different symbols for our [o]. All the symbols imply rounding, and none of them represent a really front or really low tongue position, but the variation is considerable. I believe that our identification of the "norm" of Fox [o] with the vowel of English *book* may be a minor distortion of the facts, brought about in part by our habit of thinking in standard I.P.A. terms, in part by the English phonological habits of those who have worked with Fox. I think we can correctly say that the important thing about this Fox vowel is the presence of lip involvement.

The next step in this reexamination of Fox phonology was an intuitive leap which I report only because it turned out to be a step in the direction of the new empirical point of departure to be set forth in the next section of this paper. In a game-playing mood, I decided to see what would happen if I "zeroed out" everything distinctive about Fox [e]: the articulatory properties of this unit were to be taken merely as a baseline, and the characteristics of all other bundles were to be described in terms of their departures from that baseline. ("Zeroing out" is an old Harris trick, but I have no handy reference.) Now, it is immediately apparent that we can't do *quite* what I have de-

scribed, for if we were to regard a particular recurrent unit as consisting of no distinctive articulatory components at all, we would have no way to distinguish between occurrences of that unit and the mere absence of speech. If a speaker is doing *nothing* distinctive, he is not talking. So if Fox [e] is to be taken as the baseline, then that baseline must itself be a positive entity--the only distinctive component in an occurrence of [e], but also present in every occurrence of every other bundle. I shall call the positive baseline "speech apparatus on," or, for short, *onness*.

Now, if [e] consists only of onness, the features tentatively assigned to it earlier, namely apicality and vocality, are not to be separately itemized--they are merely part of the baseline, part of what the speaker of the language produces if he turns his speech apparatus on but does nothing else.

The upshot of my experimentation was a componential analysis of Fox much simpler and neater than any I had thought of before. There are nine components:

On = *onness*: shared by all bundles, distinguishing speaking from not speaking. The only feature in [e].

Cn = *consonantality*: shared by all consonantal bundles; the only feature in [h] in addition to onness.

Sp = *spirantness*: in [s] and [š].

Na = *nasality*: in [m] and [n].

St = *stopness*: in [p], [t], [č], and [k].

Lb = *labiality*: in [o], [ō], [w], [m], and [p].

Lm = *laminality*: in [i], [ī], [y], [š], and [č].

Do = *dorsality*: in [a], [ā], and [k].

Lg = *length*: in [ē], [ā], [ī], and [ō].

Thus, synoptically;

[e	o	i	a	ē	ō	ī	ā	h	w	y	m	n	s	š	p	t	č	k]
On	On	On	On	On	On	On	On	On	On	On	On	On	On	On	On	On	On	On
	Lb	Lm	Do		Lb	Lm	Do	Cn	Cn	Cn	Cn	Cn	Cn	Cn	Cn	Cn	Cn	Cn
				Lg	Lg	Lg	Lg		Lb	Lm	Lb			Lm	Lb		Lm	Do
											Na	Na	Sp	Sp	St	St	St	St

Most of the terms are self-explanatory. "Length" involves greater relative duration, an increase of tenseness, and a modification of tongue position towards the periphery: [ē, ō, ī, ā] are roughly like the English vowels of *pad*, *note* or *boot*, *machine*, and *father* in quality, but monophthongal. Onness typically involves voicing, but that is turned off by spirantness, sometimes (optionally) by stopness, and, regardless of what features are present, towards the end of the macrosegment given certain intonations; other intonations leave it on. This last difference, as indicated, is a matter of intonation, and the

details are not known.

The dubious feature in the foregoing is the point of departure. Why choose [e] as the simplest unit, the baseline from which all other bundles depart? Why not [a]--or, for that matter, [t] or [m] or any other segment?

I have tried some of these alternatives. With the choice already illustrated, we get nine components, and we get one bundle of a single component, five of two components each, eight of three, and five of four, so that the average number of components per bundle is 2.89. If we take [a] as baseline, nine components are again enough, but there are then six bundles of three components each and seven of four each, so that the average number of components per bundle is 3. With [t] as baseline, nine components are still enough, but some bundles consist of five components, and the average number per bundle is up to 3.16.

This suggests a general principle. Let us always seek the treatment that gives the smallest number of independent variables--that is, the smallest number of components such that everything distinctive can be predicted in terms of arrangements of them. And when two or more treatments give the same number of components, let us prefer that one which yields the smallest average number of components per bundle. My choice of [e] as baseline is then no longer arbitrary, since it conforms to this general principle.

But the general principle itself is arbitrary! This is exactly the sort of development I was referring to in the initial paragraph of this paper. If our decisions have to rest on such nose-counting criteria as this, we are in a bad way.

From the various alternative interpretations of Fox phonology (or of that of any other language), I want to select that one, if any, which accounts for *more facts* than do the others. In other words, I want to discover, if possible, *objective empirical evidence in the behavior of the speakers of the language* that points to one interpretation rather than to any other. Without such evidence we are merely logic-chopping.

4. The Clues of the Base of Articulation and the Colorless Hesitation Sound. Several decades ago one of the subjects of discussion among articulatory phoneticians, particularly those concerned with the teaching of foreign languages, was the *base of articulation*. The notion goes back at least to Sievers (1876), but I must confess I did not know that until very recently. As I originally understood the matter, the proposal was that speakers of different languages hold their organs of speech in different "neutral" positions when they are not talking, and that this difference of neutral position imposes a sort of different coloring on speech. A speaker of Russian, learning English very well as far as individual consonants and vowels are concerned, and even mastering English stress and intonation, might nevertheless have a "Russian accent" in his English because his shots at well-learned English articulatory targets would be taken from the Russian neutral position instead of from that of English.

When I first tested myself on this I found it confusing. I found that when I am neither talking nor eating nor otherwise actively using my mouth its position of rest has the lips closed, the upper surface of the tongue in contact with the roof of the mouth from teeth to velum, teeth slightly separated, and velic open. That was not the "base of articulation" that anyone had described for English. It seemed to resemble more that described for Russian, and I do not have a "Russian accent" in my English (or even in my Russian).

It was many years before I realized where I had gone astray. The matter comes out quite differently if we think of the base of articulation not as the position of *rest* but as that of *readiness*.

For some languages, at least, this position of readiness can be discovered by listening for the *colorless hesitation sound*. This is the sound one sometimes produces when one is on the verge of speaking or of resuming interrupted speech but has not yet decided quite how to phrase what one is going to say. As a signal, it informs one's interlocutors of the state of affairs I have just described plus, at most, of the fact that one has taken the center of the stage and wishes to keep it.

The colorless hesitation sound must be sharply distinguished from vocal gestures that connote one or another attitude towards what is going on or what is being talked about. Thus, I have a vocal gesture of rejection and disapproval that has the quality of the vowel of *cat* or that of *man*, optionally but not always nasalized, and usually uttered with falling intonation. I have one of approval and acceptance that has the quality of the vowel of *father* (low central unrounded), not nasalized, again with falling intonation; and I have one of sympathy or commiseration that has the quality of the vowel of *law*, not nasalized, with falling intonation or with a low level pitch. None of these three is idiosyncratic, nor is any of a good many more I could describe: I hear them all from other speakers of American English, with seemingly much the same emotional values.

On the other hand, if a student has asked me a question and I am about to give an answer but not quite ready, I often produce a voiced vowel sound whose quality is much like the vowel of *fun*, not nasalized, with greatly variable duration, and on a lower mid pitch level, sometimes steady and sometimes with a slight terminal fall (in either case, the Smith-Trager pitch level /2/). This is my colorless hesitation sound. I have noted exactly the same from many fellow speakers of American English. From a few (East Coast?) the quality seems a bit lower, almost like my own vowel in *father*, but I am uncertain about this. Speakers of other languages have other colorless hesitation sounds: a colleague of mine who is a native speaker of German, for example, uses a mid front unrounded laminalized nonnasal vowel very close to the unstressed vowel of *kleine*--and he uses this whether he is speaking German or English.

I am not sure that every language has a colorless hesitation sound, or that, where such a sound exists, it is invariably indicative of the base of articulation for the language. In fact, I know one

language which has five different colorless hesitation sounds; clearly, at most one of these can point to the base of articulation. Fijian has vowels [i, e, a, o, u], and every syllable ends with one or another of them. If a speaker interrupts what he is saying to think of what to say next, he merely prolongs whichever of the five vowels he has just produced. I don't know what the *initial* hesitation sound is--that is, the sound a Fijian would produce (if any) while thinking how to *start* an utterance. I rather suspect that it is a higher low central unrounded vowel, like the unstressed Fijian [a], and that this is indeed indicative of the base of articulation. But my grounds for this suspicion are very tenuous.

However, if colorless hesitation sounds are not universal, or are not invariably a reliable guide, I am nevertheless convinced that each language--or, perhaps I had better say, each dialect, or even each idiolect--does involve a base of articulation, the character of which can be determined by appropriate observation. Assuming that this is so, I can now state my proposed new point d'appui for phonology.

I propose: that the base of articulation for any language is its phonological baseline; that all articulatory targets for the language are definable in terms of departures from that baseline; that the articulatory properties of the baseline constitute mere onness, present during all speech in the language; and that the recurrent segment during actual speech that most nearly resembles the baseline likewise consists of onness with no additional distinctive components.

Consider an analog: the playing of a musical instrument, say a violin. When a violinist is walking, driving a car, eating, sleeping, or otherwise engaged in nonviolinistic activities, the positions and motions of his arms, hands, and head bear no obvious testimony to his special motor skills as a musician; similarly, the position of *rest* of a person's speech organs hardly make it possible to guess what language he speaks. But when the violinist picks up his instrument and bow to play, he arranges them, and the parts of his body, in a position quite different from any position any of us assume for any other activity. It is, in fact, a very tiring twisted position for the nonviolinist, as any beginner on the instrument can testify; but it is a maximally economic base position for the extremely precise and rapid motions that must be made in playing the instrument. This base position has evolved over a number of centuries of violin artistry, and is passed on to each new would-be violin player. Similarly, when a user of a language is just about to speak, he places his speech organs in an economic base position for the motions that must be made in speaking the particular language. That base position changes, over the centuries, as the phonological economy of the language changes; it is passed on to each new generation of speakers as part of the phonological economy.

This proposal replaces the analyst's subjective feeling for simplicity, symmetry, or elegance by reliance on an objective fact observable in the actual behavior of the users of the language. In so doing, it reduces the amount of arbitrariness in phonological theory

and technique.

But let me add immediately that the gain is limited. I am certainly not claiming that the new point of departure solves all problems--in fact, it may reveal some we didn't know we had.

Consider Fox again. I'm sorry to say I don't know what the colorless hesitation sound is in Fox, nor the character of the Fox base of articulation. The componential analysis given in section 3 is empirically justified only if the base of articulation has the properties of [e]. Let me here assume that that is so, in order to demonstrate that a measure of arbitrariness would remain:

(1) The new point of departure does not tell us whether to interpret the long vowels as single bundles (they are so treated in section 3) or as geminate clusters. The latter alternative would eliminate four columns from the table and would get rid of the component "Lg". I prefer the treatment as single bundles, but the reasons have nothing to do with the new point d'appui.

(2) By a slight redefinition of "Sp" we could assign that feature to [h] as well as to [s] and [š]. We could devise a feature "Sh" (*shortness*), the presence of which would distinguish [w] and [y], respectively, from [o] and [i] (it was done that way in Hockett 1955 pp 129-133). Then the feature "Cn" (consonantality) could be left off the table; it would be reduced to determined status, since any bundle containing shortness, nasality, spirantness, or stopness would be consonantal and no other bundle would be.

In the light of the foregoing, it should be clear that I am by no means confident about the following tentative treatment of my own variety of American English. The point of departure is clear: my hesitation sound is like the vowel of *cup*, and this is indicative of the base of articulation; so this vowel consists of nothing but onness. Onness entails voicing, vocalicity, mid tongue height, dorsality, and orality (absence of nasality); these properties are thus "zeroed out." The treatment is directly in terms of allophonelike bundles, rather than in terms of "phonemes" as previously determined by some other approach. As for Fox, juncture, stress, and intonational features are left out of account. The components are:

On = *onness*, already described.

Lo = *lowness*, for low vowels.

Hi = *highness*, for high vowels.

Ap = *apicality*.

Lm = *laminality*.

Lb = *labiality*.

Rt = *retroflexion*.

Lt = *lateralness*.

Na = *nasality* (with an oral closure).

Cn = *consonantality.*

Vl = *voicelessness.*

Fr = *fricativity.*

Sb = *sibilance.*

St = *stopness.*

In the top line of the following table a prime is an ad-hoc diacritic for laminalization (as discussed towards the beginning of §3) or laminality; a subposed dot means syllabic. In the second line, the schwa with a curve under it is the centering glide of such diphthongs as that in *yeah*. The [t′] and [d′] in the third line occur respectively only as the first parts of the affricates in *church* and *judge*:

[ə	a	ɨ	e	æ	i	e′	æ′	i′	o	ɔ	u	r̩	l̩	m̩	n̩]
On	On	On	On	On	On	On	On	On	On	On	On	On	On	On	On
	Lo	Hi	Ap	Ap	Ap	Lm	Lm	Lm	Lb	Lb	Lb	Rt	Lt	Lb	Ap
				Lo	Hi		Lo	Hi		Lo	Hi			Na	Na

[ə̯	w	y	r	l	m	n	ŋ	h]
On	On	On	On	On	On	On	On	On
Cn	Cn	Cn	Cn	Cn	Cn	Cn	Cn	Cn
	Lb	Lm	Rt	Lt	Lb	Ap	Na	Vl
					Na	Na		

[v	ð	z	ž	f	θ	s	š	b	d	d′	g	p	t	t′	k]
On	On	On	On	On	On	On	On	On	On	On	On	On	On	On	On
Cn	Cn	Cn	Cn	Cn	Cn	Cn	Cn	Cn	Cn	Cn	Cn	Cn	Cn	Cn	Cn
Lb	Ap	Ap	Lm	Lb	Ap	Ap	Lm	Lb	Ap	Lm	St	Lb	Ap	Lm	St
Fr	Fr	Sb	Sb	Fr	Fr	Sb	Sb	St	St	St		St	St	St	Vl
				Vl	Vl	Vl	Vl					Vl	Vl	Vl	

The foregoing leaves altogether out of account the matter of (sequential) limitations on arrangement. For example, codas [ts] and [dz] occur, but [tz] and [ds] do not. This suggests that the structure of such a microsegment as *pots* is not as shown on the left below, but as shown on the right:

On	On	On	On
Cn	Lo	Cn	Cn
Lb		Ap	Ap
St		St	Sb
Vl		Vl	Vl

On			
Cn	Lo	Cn	
Lb		Ap	
St		St	Sb
Vl		Vl	

That is: some component-occurrences last longer than others. Limitations on arrangement are both simultaneous and sequential, and one's analysis of a sound system is not complete until all have been taken into consideration. I do not see that the new point d'appui renders this any easier, but maybe it helps us be a bit more realistic.

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THE DEGREE OF DIFFERENCE AND PERCEPTUAL CONFUSIONS

Dale E. Woolley

University of Illinois at Chicago Circle

(1) Introduction

The theory of degrees of difference, first developed by Sol Saporta (1955) and subsequently worked out in further detail by Lee S. Hultzén (1965), provides a convenient quantitative measure of the difference between two phonological units in a language. For both Saporta and Hultzén, the degree of difference is a measure of the distance between two phonemes in the structural pattern of the phonemic system of a language. But Hultzén implies also that the system has validity as a measure of the perceptual difference between phones. Attractive as the theory is, it has so far remained only theory. Although there exist a few studies, primarily the classic paper by Miller and Nicely, analyzing the confusions that occur between phones in perception, no effort has yet been made to determine whether the theory of degrees of difference can be verified by this type of objective data.

In Hultzén's system, for instance, there are two degrees of difference between /s/ and /š/, three between /s/ and /θ/, and four between /s/ and /f/. The implication in his discussion is that /s/ is more similar to /š/ than to /θ/ or /f/ in perception. Thus when /s/ is misidentified in speech, it should be heard more often as /š/ than as /θ/ or /f/.

This study evaluates the theory of degrees of difference and determines to what extent degrees of difference may be used as predictors of perceptual confusion among the so-called r-vowels in English.

Although there are a number of studies analyzing perceptual confusions among consonants, there are only a few treating confusions among vowels. Peterson and Barney (1952) and Fairbanks and Grubb (1961) provide vowel identification matrices. In both of these investigations, judges were asked to identify samples of recorded vowels so that the best samples could be selected for acoustic analysis. Since the speech in these studies was presented to listeners without interference, the intelligibility scores are high. Misidentifications, which are few in number, occur between vowels which are contiguous on an F1-F2 plot or on the vowel triangle. They result from small differences in the frequency of F1 or F2 (or, put another way, in the configuration of the vocal tract) and represent the last in a series of independent hierarchical phonetic parameters.

In order to uncover the other parameters underlying the perception of vowels, it is necessary to interfere in some fashion with the speech signal. Curry (1950) studied the confusions which occurred when syllables consisting of nine English vowels in the environment

/t_p/ were identified by sixteen listeners at low intensities. He found that most of the confusion occurs between vowels next to one another on the vowel triangle. But he also noticed that /i/ and /u/ are confused about 50% of the time. Curry's study seems to suggest that a feature involving F1 alone may be at work.

That a feature of length in addition to F1 and F2 might be distinctive for vowels was suggested by Miller (1956), who analyzed confusions which occurred when sixteen English vowels and diphthongs were subjected to low-pass filtering. He found that among vowels with similar F1 frequencies, the long vowels are confused with each other the short vowels with each other, but that long and short vowels are not confused. Berry (1972) has recently reconfirmed this finding in a similar study.

Wickelgren (1965) has extended the analysis of confusion to determine the way in which phonemes are coded in short-term memory. He employed lists of syllables containing the vowels /ɩ, ɛ, æ, ʊ, ʌ, ɑ/. These syllables were recorded by informants and played to listeners, who first wrote down the syllables they heard and then covered their responses and re-wrote them from memory. The only recall responses subjected to analysis were those corresponding to syllables which had originally been identified correctly from the recordings. This procedure eliminated errors in perception from the data.

Wickelgren found that recall errors for vowels are related to, and best predicted by, a phonological feature system consisting of two places of maximum constricition in the vocal tract (his term for the more traditional rubrics front and back) and three degrees of openness of the vocal tract (close, mid, or open in traditional terminology).

More recently several studies exploiting the method of similarity judgment have been reported. Mohr and Wang (1968) presented recordings of /i, e, æ, y, ø, œ, u, o, ɔ/ in all possible paired combinations to listeners who judged the degree of similarity between the vowels in each pair on a six-point scale. Singh and Woods (1971) studied twelve American vowels using essentially this same technique. Pols, et al. (1969) used the method of triadic comparison, wherein the most and least similar pair of vowels is selected from three-vowel sets, to study eleven Dutch vowels. These studies suggest that judgments of vowel similarity involve horizontal tongue displacement (front-central-back in articulatory terms or the difference between F2 and F1 in acoustic terms), rounding (lowering of F2 and F3), and tongue height (the inverse of F1). See Ladefoged (1975, pp. 258-9).

(2) The Calculation of Degrees of Difference

In order to relate complexity of construction of consonant clusters to their relative frequency of occurrence, Saporta (1955) needed a quantitative measure of the difference between two phonemes and so devised the degree of difference. In his scheme this measure is derived from a comparison of the Jakobsonian distinctive feature config-

urations of each phoneme. The difference between a plus for any feature of one phoneme and a minus for the same feature of another phoneme is counted as two. The difference between a plus or a minus for any feature of one phoneme and no specification for that feature in another phoneme is counted as one. A feature is not specified for a phoneme when it is nondistinctive for that phoneme, or when it is redundant because it is predictable from other feature specifications for that phoneme. If, for example, /X/ in some language has the features - vocalic, + consonantal, + tense, - interrupted, + strident, + compact, and /Y/ has the features - vocalic, +consonantal, + tense, + interrupted, - strident, - acute, then there are <u>six</u> degrees of difference between them. Both phonemes have different specifications for the features interrupted and strident, yielding a total of four degrees of difference; /X/ is + compact and has no specification for acute, while /Y/ is - acute and has no specification for compact, yielding two additional degrees of difference.

Hultzén (1965) employs Saporta's basic method of calculation, but his values are considerably different because he does not always count the difference between a specification for a feature in one phoneme and no specification for that same feature in another. The treatment of redundant (unspecified) features is crucial in the calculation of degrees of difference. Hultzén's arrangement and calculations clearly show that he understands this fact, but he does not discuss it in detail.

The set of <u>r</u>-vowels in English will serve as an example for the discussion that follows. Any set of phonemes would work just as well, but since the <u>r</u>-vowels are the subject of an important part of this paper, it seems profitable to employ them here.

Table 1. Distinctive Feature Matrix and Degrees of Difference for <u>r</u>-vowels in GA English

	ʊɚ	iɚ	ɔɚ	ɝ	ɛɚ	ɑɚ	ʊɚ					
Diffuse	+	+	-	-	-	-	6	iɚ				
Compact	-	-	-	-	-	+	2	8	ɔɚ			
Grave	+	-	+	-	-	-	6	4	4	ɝ		
Acute	-	+	-	-	+	-	8	2	6	2	ɛɚ	
Flat	+	-	+	-	-	-	8	6	6	2	4	ɑɚ

Table 1 displays a distinctive feature representation[1] for the

[1]Calculations could, of course, be based on the currently more fashionable Chomsky-Halle articulatory features, but as Fudge (1973,p.174) has

r-vowels and a table of degrees of difference calculated from the matrix. It should be apparent that the r-vowels are here considered vowel nuclei and not VC sequences, and that /ɝ/ is considered a mid-central vowel. The distinctive feature complexes clearly contain redundancies. For instance, the specifications for the features grave and flat are identical, /ɑɚ/ is the only + compact and /ʊɚ, ɪɚ/ are the only diffuses.

In addition to the obvious requirement that, like all scientific descriptions, phonological analyses account accurately for the objective (observable phonetic) facts, it is also desirable that, as Halle (1964) says, they provide reasonable, insightful, general, simple, and economical statements about the facts. Thus, to achieve economy and simplicity of statement, redundancy should be eliminated from distinctive feature representations, consistent with maintaining an accurate and reasonable analysis. One method of treating redundancy (that employed by Jakobson, Fant, and Halle, 1969, p.44) is to eliminate it.

The problem of eliminating maximum redundancy can be viewed as one of finding n, the minimum number of binary specifications necessary to keep k phonemes apart, given a framework of m phonological features. The highest value of n occurs when m=1. In this circumstance, each phoneme must be enumerated in the binary system. The lowest value of n occurs when m=k=n and each phoneme is kept distinct by a different feature. In the example used here, k is six; there are six vowel nuclei under consideration. If m were one, that is, if only one feature, say vowel, were employed to keep the six nuclei apart, then each nucleus would be assigned a three-digit binary number. A total of eighteen specifications, the highest value for n, would be needed. If m were six, that is, if the phonological system employed six features, and if each phoneme were marked plus for a different feature, then only six specifications, the lowest value of n, would be needed. See Table 2.

Table 2. Highest and Lowest Values of n when k = 6

	k_1	k_2	k_3	k_4	k_5	k_6
m	001	010	011	100	101	110
			n = 18			

	k_1	k_2	k_3	k_4	k_5	k_6
m_1	+					
m_2		+				
m_3			+			
m_4				+		
m_5		n = 6			+	
m_6						+

pointed out the Jakobsonian acoustic features have particular relevance for perception.

In natural languages n is located somewhere between the highest and lowest possible values of n because the phonological features and specifications are not arbitrarily assigned but are related to the phonetic facts of the language in question. Consider the example of the r-vowels again. Table 1 reflects the organogenetic facts that /ʊɚ, ɪɚ/ are close vowels, /ɔɚ, ɝ, ɛɚ/ are mid vowels, /ɑɚ/ is an open vowel, /ɪɚ, ɛɚ/ are front vowels, /ʊɚ, ɔɚ/ are back rounded vowels, and /ɝ/ is a central vowel. Put another way, the phonology of no language involves only one phonetic parameter or employs as many features as there are phonemes in the language, with each phoneme marked for a different feature.

Table 3. Nonredundant Distinctive Feature Matrix and Degrees of Difference for r-vowels in GA English

	ʊɚ	ɪɚ	ɔɚ	ɝ	ɛɚ	ɑɚ						
Diffuse	+	+	–	–	–		ʊɚ					
Compact				–		+	2	ɪɚ				
Grave	+	–	+	–			2	4	ɔɚ			
Acute				–	+		6	4	4	ɝ		
							4	4	2	4	ɛɚ	
							3	3	3	5	3	ɑɚ

Table 3 displays the distinctive feature representation for r-vowels when maximum redundancy is eliminated consistent with a defensible phonological representation and restorability by rule. The features in the matrix have thereby been reduced from five to four and the number of specifications from thirty to thirteen.

Another method of eliminating redundancy has been suggested by Cherry, Halle, and Jakobson (1953) and first employed by Halle (1959) in his analysis of Russian phonology. The purpose of this method is to remove redundancy from the distinctive feature matrix in such a way that the phonemes may be identified by successive binary decisions. For Cherry, et al., the process of perception of phonemes involves a series of binary choices. The listener decodes the messages by unconsciously asking a series of questions: is the phoneme under consideration vowel or consonant? is it interrupted or continuant?

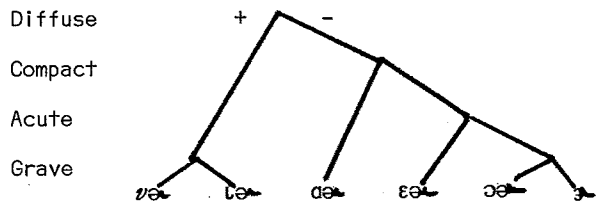
Table 4. Partially Redundant Distinctive Feature Matrix Arranged for Binary Identification of r-vowels in GA English and the Associated Table of Degrees of Difference

	ʊɚ	ɪɚ	ɔɚ	ɝ	ɛɚ	ɑɚ						
Diffuse	+	+	–	–	–	–	ʊɚ					
Compact			–	–	–	+	2	ɪɚ				
Acute			–	–	+		4	6	ɔɚ			
Grave	+	–	+	–			6	4	2	ɝ		
							5	5	3	3	ɛɚ	
							4	4	4	4	3	ɑɚ

The arrangement in Table 4 employs seventeen feature specifications, four more than necessary for unambiguous representations of each phoneme, but it also provides for the identification of each phoneme by a series of binary decisions. For instance, /ɔɚ/ could be identified by a listener or a phonological rule with four questions. Is it diffuse? No. Is it compact? No. Is it acute? No. Is it grave? Yes.

The data in the matrix in Table 4 can also be displayed on a branching tree diagram (Figure 1). Various routes through the tree graphically indicate the identification of phonemes by successive binary decisions.

Figure 1. Distinctive Feature Coding for r-vowels in GA English Displayed on a Branching Tree Diagram



Consider now the tables of degrees of difference as presented in Tables 1, 3, and 4. The importance of redundant features in the calculation of degrees of difference is apparent. The calculations clearly yield quite different results depending on whether they are based on redundant, or nonredundant, or partially redundant treeable distinctive feature matrices. It is important to notice also that the results differ not only in magnitude but also in rank order of magnitude. For instance, in Table 1 there are six degrees of difference between /ʊɚ/ and /ɪɚ/ and two between /ɝ/ and /ɛɚ/, whereas in Table 3 there are two between /ʊɚ/ and /ɪɚ/ and four between /ɝ/ and /ɛɚ/. In Table 3 there are two degrees of difference between /ɔɚ/ and /ɛɚ/ and four between /ɔɚ/ and /ɝ/, whereas in Table 4 there are three between /ɔɚ/ and /ɛɚ/ and two between /ɔɚ/ and /ɝ/.

Another problem, associated with the elimination of redundant features and affecting the calculation of differences, obtains when alternative orderings of features in the matrix are possible. If differences are based on a fully specified matrix such as that in Table 1, there is no difficulty. Any order will result in the same values. If, however, calculations are based on tables with redundancy eliminated, the ordering of features is critical. Consider, for example, the tense stops in English.

	p	t	k		p	t	k		p	k	t
Compact	-	-	+	Compact	-	-	+	Grave	+	+	-
Grave	+	-	+	Grave	+	-		Compact	-	+	

The arrangement above displays three distinctive feature matrices for these stops. In the matrix on the left, the features compact and grave are fully specified for each stop. The other two are treeable matrices, each of which has a redundant feature removed. Furthermore, the order of the features is reversed. When the order is compact followed by grave, /k/ is three degrees away from both /p/ and /t/, which are themselves two degrees apart. With the reverse ordering, that is grave followed by compact, /t/ is three degrees away from both /p/ and /k/, which are themselves two degrees apart.

It is necessary now to determine whether the various calculations of degrees of difference--based on the choice of ways in which redundant features can be handled--also affect, in turn, the prediction of perceptual confusions. Shown below are four distinctive feature matrices for English tense stops. Only those features which serve to differentiate the stops are under consideration.

Table 5. Distinctive Feature Matrices for Tense Stops in English

	k	p	t		k	p	t
Compact	+	-	-	Compact	+		-
Grave	+	+	-	Grave		+	
Compact	+	-	-	Grave	+	+	-
Grave		+	-	Compact	+	-	

In the matrix on the upper left, all redundant features are included. When degrees of difference are calculated from this matrix, both /k/ and /t/ are two degrees from /p/ and four degrees from each other. In the matrix on the upper right, no redundant features are included. When calculations are made from this nonredundant matrix, all the phonemes are two degrees apart. The other two matrices are partially redundant and treeable. When the matrix on the lower left is used for calculation, /k/ is three degrees from both /p/ and /t/, which are themselves two degrees apart. When the matrix on the lower right is used, /t/ is three degrees from both /k/ and /p/, which are themselves two degrees apart.

Clearly when three phonemes are separated by two binary features, only partially redundant treeable matrices provide for a grouping of two of the phonemes in contrast to the third phoneme, which can thus be separated from the more closely related pair. Woolley (1968) applies the theory of degrees of difference to the prediction of consonantal confusions as they appear in the confusion matrices of Miller and Nicely (1955) and Pollack and Decker (1960). The Miller-Nicely confusion matrices indicate that in perception /k/ and /p/ tend to form a confusion pair separated from /t/. This fact is correctly predicted by a system of degrees of difference based on the matrix on the lower

right in Table 5. Likewise, the Pollack-Decker confusion matrices show that /r/ and /w/ remain interconfused at higher speech-to-noise ratios and are distinguished from /j/. Woolley argues that such phenomena suggest that many perceptual confusions can best be predicted by systems of degrees of difference calculated from partially redundant treeable matrices.

A more thorough examination of perceptual confusions indicates, moreover, that certain additional redundant or nondistinctive features sometimes need to be reintroduced if accurate predictions are to be achieved. The nasals, the glides, and /l/ in English are nondistinctively nontense (lax). Hultzén (1965), in his system of degrees of difference, reintroduced this nondistinctive feature for these phonemes in order to place them one degree closer to the distinctively nontense phonemes in his system. Corroboration of Hultzén's method is evidenced in the Miller-Nicely data showing that when /m/ or /n/ is misidentified as a non-nasal phoneme, that phoneme will more frequently be nontense than tense.

Another situation in which a redundant or nondistinctive feature needs to be reintroduced to adequately predict perceptual confusions occurs with the noninterrupted obstruents in English. For example, consider only the tense subset / š, s, f, θ/, whose treeable matrix is shown in Table 6. Woolley (1968a) argues that English /f, v/ are nonstrident.

Table 6. Distinctive Feature Matrix and Degrees of Difference for Noninterrupted Tense Obstruents in English

	š	s	f	θ								
					š				š			
Strident	+	+	-	-	2	s			2	s		
Grave		0	+	-	4	4	f		5	4	f	
Compact	+	-	0		4	4	2	θ	4	3	2	θ

The zero indicates a redundant or nondistinctive minus for a feature and is counted as different from a plus, but not different from a minus or an unspecified feature. The calculations in the first table of degrees of difference are based on the matrix without the zeros; those in the second table, with the zeros. Hultzén (1965) suggested indicating the redundant specification for /s/. The addition of 0 compact for /f/ provides degrees of difference which reflect the perceptual confusions in the Miller-Nicely data very well.

Woolley (1968) has also shown that in addition to a procedure for handling redundant or nondistinctive features, a system of calculating degrees of difference must provide for a hierarchy of phonological features. Ladefoged (1970) has made the same point. For instance, the Miller-Nicely data show that at low speech-to-noise ratios and in narrow band low and high pass filtering systems, nasality and tension (voicing) are easily perceived. That is, perceptual confusions tend to occur between the nasal consonants, or among the tense consonants, or among the nontense consonants. But very few confusions occur between nasal

phonemes and tense phonemes, or between tense phonemes and lax phonemes. As the speech-to-noise ratio is raised, or as the frequency bandwidth of the filtering conditions is increased, fewer confusions occur between interrupted and noninterrupted phonemes, strident phonemes and nonstrident phonemes, and so forth. That a perceptual hierarchy of phonological features exists has been reconfirmed in a number of other studies. Cf. Singh and Black (1966), Greenberg and Jenkins (1964), and Mohr and Wang (1968).

(3) The Prediction of Perceptual Confusions Among r-vowels.

Woolley (1968) reports a study in which nonsense syllables containing the so-called r-vowels in English--the mid-central-molar-monophthongal /ɝ/ in purr and the centering diphthongal /ɪə, ʊə, ɛə, ɔə, ɑə/ in peer, poor, pear, pour, par--in five phonetic environments representative of the phonotactics of English were recorded by informants and then played back under five conditions of white masking noise to listeners who attempted to determine which syllables they heard. The data were displayed in conventional confusion matrices for analysis. When the confusion matrices are examined in detail, one observes first that /ɑə/ is in almost every testing condition (i.e. at every S/N (speech-to noise ratio)) isolated from the other five vowels. Second, one notices that /ɪə/ and /ʊə/ form a strong interconfusion pair in all the matrices. At higher speech-to-noise ratios the interconfusions between /ɪə/ and /ʊə/ weaken. Third, /ɛə/ and /ɔə/ also form an interconfusion pair differing from the pair formed by /ɪə/ and /ʊə/ in that it has a tendency to weaken at lower speech-to-noise ratios. Fourth, the complexity of confusions involving /ɝ/ presents difficulties in analysis. This vowel enters into strong confusion relationships with all the diphthongs except /ɑə/. These confusions can be seen to form patterns with the following general characteristics: (1) the four vowels /ɪə, ʊə, ɛə, ɔə/ are often strongly misidentified as /ɝ/; (2) /ɝ/ is often strongly misidentified as /ɪə, ɛə, ɔə/, but only infrequently as /ʊə/; (3) at lower speech-to-noise ratios /ɝ/ enters into confusion patterns with the interconfusion set of both /ɪə/ and /ʊə/ and /ɛə/ and /ɔə/; (4) at higher speech-to-noise ratios /ɝ/ enters into confusion patterns with /ɛə/ and /ɔə/ in but one environment, but with /ɪə/ and /ʊə/ in the other environments. Lastly, two other patterns of confusion need to be mentioned. Only seven instances of significant confusions between /ʊə/ and /ɔə/ and between /ɪə/ and /ɛə/ appear in the matrices. These instances are all one-directional and tend to occur at the higher speech-to-noise ratios.

Perhaps the most striking result of the analysis of perceptual confusions among the r-vowels is the uncovering of three principal confusion sets corresponding to the traditional analysis of vowels as close, mid, and open in articulatory phonetics. The first set consists of only one member, /ɑə/; the second consists of /ɛə/ and /ɔə/ and sometimes /ɝ/; the third consists of /ɪə/ and /ʊə/ and sometimes also /ɝ/. It is also important to note that relatively few confusions occur between /ɪə/ and /ɛə/, the front vowels, and between /ʊə/ and /ɔə/, the back vowels. Finally, since different phonetic environments have neglible effect on the overall patterns of confusion, the results of the tests can be pooled and displayed in the confusion matrix in Table 7.

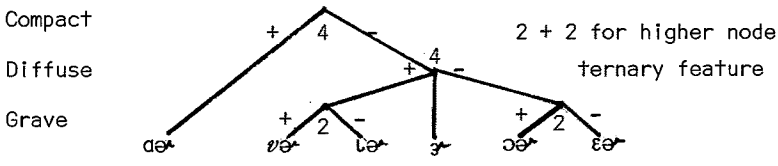
Table 7. Pooled Confusion Matrix for r-vowels in Five Environments and Five Masking Conditions

Vowel Spoken	Vowel Identified: ɪər	ʊər	ɜr	ɛər	ɔər	ɑər	NR	PC	T	TC
ɪər	1341 66.3	346 17.1	115 5.7	68 3.4	44 2.2	23 1.1	85 4.2	1687 83.4	2022	
ʊər	427 21.2	1038 51.3	288 14.2	43 2.1	114 5.6	17 0.8	95 4.7	1753 86.7	2022	
ɜr	167 8.3	87 4.3	1386 68.6	165 8.2	93 4.6	28 1.4	96 4.7	1386 68.6	2022	
ɛər	99 4.9	27 1.3	164 8.1	1394 68.9	171 8.5	92 4.6	75 3.7	1394 68.9	2022	
ɔər	24 1.2	89 4.4	113 5.6	172 8.5	1510 74.7	58 2.9	56 2.8	1510 74.7	2022	
ɑər	28 1.4	10 0.5	37 1.8	60 3.0	35 1.7	1797 88.9	55 2.7	1797 88.9	2022	
T	2086	1597	2103	1902	1967	2015	462		12132	8466

NR= no response PC= total principal confusions T= total
TC= total correct The entries in each cell represent responses expressed by number and percent.

Several plausible distinctive feature arrangements for the r-vowels have been examined in detail. The system shown in Figure 2 best reflects the patterns of confusion among the r-vowels.

Figure 2. Phonemic Inventory of r-vowels Displayed on a Branching Tree



It will be recalled that /ɑər/ is kept separate in perception from the other vowels at the lowest speech-to-noise ratios. Strong interconfusion pairs are formed between /ɪər/ and /ʊər/ and between /ɛər/ and /ɔər/ at most speech-to-noise levels investigated. And /ɜr/ enters into confusions with both /ɪər/-/ʊər/ and /ɛər/-/ɔər/. The feature compact thus separates /ɑər/ from the other vowels. The feature grave separates /ʊər/ from/ɪər/ and /ɔər/ from /ɛər/. And the feature diffuse becomes ternary

to properly place /ɝ/ between /vɚ/-/ɪɚ/ and /ɔɚ/-/ɛɚ/.

The distinctive feature matrix corresponding to the branching tree diagram in Figure 2, along with a table of degrees of difference calculated from it, appears in Table 8. The 0 indicates a minus nondistinctive feature, the x a plus nondistinctive feature. A 0 or x for any feature counts two different from a specification of opposite value for that feature, one different from a ± for that feature, one different from another nondistinctive specification of opposite value for that feature, not different from a specification of the same value for that feature, and not different from no specification for that feature. The specifications for flat are reintroduced in order to place /ɝ/ closer to /ɪɚ, ɛɚ/ than to /vɚ, ɔɚ/.

Table 8. Distinctive Feature Matrix and Degrees of Difference Underlying Perceptual Confusions Among r-vowels

	ɑɚ	vɚ	ɪɚ	ɝ	ɔɚ	ɛɚ
Compact	+	-	-	-	-	-
Diffuse	0	+	+	±	-	-
Grave	0	+	-		+	-
Flat		x		0	x	

	ɪɚ	vɚ	ɝ	ɛɚ	ɔɚ	ɑɚ
ɪɚ	2					
vɚ	3	4				
ɝ	4	6	3			
ɛɚ	6	4	4	2		
ɔɚ	6	8	5	4	6	

The difference between + diffuse and - diffuse counts four degrees instead of three to maintain integers throughout the system of degrees of difference. Thus the difference between + diffuse or - diffuse and ± diffuse counts two. The hierarchy of features is taken into account by counting the difference between + compact and - compact as four degrees.

For example, there are six degrees of difference between /ɑɚ/ and /ɔɚ/. The + compact of /ɑɚ/ and the - compact of /ɔɚ/ count four degrees. Because 0 represents a minus nondistinctive feature, the 0 diffuse of /ɑɚ/ is not different from the - diffuse of /ɔɚ/, but the 0 grave of /ɑɚ/ is two degrees different from the + grave of /ɔɚ/. The x flat of /ɔɚ/ is not different from the unspecified flat of /ɑɚ/. The total, then, is six degrees of difference: four provided by the compact feature, and two by the grave feature.

This system of degrees of difference correctly predicts the rank order of misidentifications in 100% of the cases displayed in Table 7, which, for instance, shows that /ɝ/ was misidentified as /ɪɚ/ 167 times, as /ɛɚ/ 165 times, as /ɔɚ/ 93 times, as /vɚ/ 87 times, and as /ɑɚ/ 28 times. The frequencies and rank order of these misidentifications is accurately predicted by the system of degrees of difference developed in this study. /ɝ/ is three degrees from /ɪɚ/ and /ɛɚ/,

four degrees from /ʊə/ and /ɔə/, and five degrees from /ɑə/.[2]

The numbers in the system of degrees of difference are relative rather than absolute. That is, they essentially predict the rank order in which misidentifications for a given stimulus will occur. Two degrees separate /ɪə/ from /ʊə/ and /ɛə/ from /ɔə/, respectively. Thus /ɪə/ should be the most frequent misidentification for /ʊə/; /ʊə/, the most frequent misidentification for /ɪə/; /ɛə/, the most frequent misidentification for /ɔə/; and /ɔə/, the most frequent misidentification for /ɛə/. These predictions are in fact supported by the data. That the numbers in the system are relative, however, is reflected by the fact that over twice as many misidentifications occur between /ɪə/ and /ʊə/ as between /ɛə/ and /ɔə/.

When this system of degrees of difference is compared with the perceptual confusions in the summary matrices for each speech-to-noise ratio tested in Woolley (1968), 142 out of 180 predictions or 82% are correct.

The success of the prediction of perceptual confusions among the r-vowels by means of the system of degrees of difference developed in this study may be compared with the success of the prediction of errors in short-term memory reported by Wickelgren (1965). His feature analysis correctly predicts 100% of the recall errors. But for a correct prediction he requires only that vowels sharing a feature with a stimulus vowel be incorrectly recalled more frequently than those which do not share a feature with the stimulus vowel. In order to compare Wickelgren's results with those obtained for the r-vowels in this study, however, one must derive a system of degrees of difference from his analysis which will predict a rank ordering of the frequency of recall errors. By this more stringent criterion, the prediction of recall errors of vowels in each of the two environments he investigated is only 67% correct. Similarly, only 73% of his pooled recall errors are correctly predicted. If the same standards are used to judge both sets of data, then Wickelgren's predictions of recall errors would clearly be less successful than the predictions of perceptual errors among the r-vowels in this study.

(4) Implications for the Hierarchical Organization of Phonological Features.

Studies such as the one just reported are typically interpreted in two ways: the results are related in so far as possible to the phonetic facts, both articulatory and acoustic, that might explain them, and inferences about perception are drawn wherever possible. This study

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[2]If the cells in this matrix are summed diagonally so that the interconfusions between pairs of vowels are pooled, the scores with respect to /ɝ/ are 375 /ʊə/, 329 /ɛə/, 282 /ɪə/, 206 /ɔə/, and 65 /ɑə/. Removing the redundant flat feature would reduce the differences between /ɝ/ and /ʊə/ and /ɔə/ respectively to 3 degrees, providing good prediction. Pooled scores for the other vowels are correctly predicted.

corroborates those of Miller (1956) and Berry (1972) in demonstrating the perceptual relevance of tongue height in judgments of vowel quality. Both Miller and Berry presented vowels to listeners under low-pass filter conditions. Here vowels were presented behind random noise. The effect of low-pass filtering is to remove the second and higher formants from the acoustic signal. The effect of random noise is to obscure the acoustic signal, but since the amplitude of the signal in the second and higher formants is considerably lower than that in F1, the second and higher formants are effectively obliterated. It is well known that F2 contributes to front-back distinctions in vowels, whereas F1 supplies the principal cues for height distinctions. Thus without information provided by F2, front-back distinctions collapse and vowel judgments are made along the tongue height dimension. Put another way, the results of studies such as those discussed here is entirely predictable on phonetic grounds.

It is tempting to try to find in studies of perceptual confusion evidence not only for specific phonological parameters, but also for a hierarchical arrangement of such parameters. Phoneticians and phonologists would generally agree, I think, that such a hierarchy exists. Such a view is at least implicit in a large body of work. We would like a phonetic theory which would provide an internally consistent, insightful, explanatory, and principled determination of such a hierarchy. To my knowledge, no such theory has been proposed. Ladefoged's (1975) theory entails the best set of features I have seen proposed, but they are not hierarchical in organization. We therefore have had to infer such a hierarchy from less direct sources, for example, the phonologies of specific languages, phonological universals, phonological acquisition, speech pathology, and perception.

Woolley (1968) incorrectly argued that data from confusion studies such as those here under discussion disclose a feature hierarchy. Such data can, of course, be interpreted in terms of a hierarchy, but they do not themselves necessarily imply a hierarchy. All they can tell us is what some of the parameters are. Berry (1972) also presented his vowels to listeners under high-pass filter conditions with the predictable result that confusions occurred along the horizontal tongue displacement dimension.

A more reliable source of perceptual evidence for a feature hierarchy comes from similarity studies such as those cited earlier in the paper. Pols, et al. (1969), Singh and Woods (1971), and Terbeek (1973) subjected their data to multiple-scaling analyses, which uncover features, but not a hierarchy. Mohr and Wang (1968) and also Terbeek (1973) interpret their data in terms of the marked/unmarked distinction. However, a careful examination of the raw data, as displayed in similarity matrices, from such studies does offer indications for a feature hierarchy, although interpretation is difficult. Specific features behave differently with respect to similarity judgments within that feature, rank ordered with respect to decreasing similarity as nasality, rounding, front, central, back, and height. For example, similarity judgments among /ĩ, ɑ̃, ũ/ are greater than those among /i, ɑ, u/, etc. Similarity measures between vowels which differ by more than one feature are more difficult to interpret. The results of the three studies we have are not unambiguous, but the following seem to be tendencies: (1) a vowel

is judged to be most similar to other vowels which share its horizontal tongue displacement and rounding except that the lowest vowels have a strong affinity for other low vowels of the same rounding. (In the Pols, et al. data, however, the height feature is paramount.) (2) Vowels differing in rounding are judged to be very similar if they share displacement. (3) Vowels differing in both rounding and displacement are judged to be somewhat similar if they share height. Examples of the best and worst approximations to these tendencies are /i/ judged as decreasingly similar to /e, æ, y, ø, oe, u, o, ɔ/ in that order and /y/ as /ø, u, oe, e, i, o, ɔ, æ/. Although the exact hierarchical position of rounding cannot be inferred from these data because no back unrounded vowels were studied, the data as a whole suggest that nasality, rounding, tongue displacement, and tongue height are hierarchically structured.

It is interesting to note that from the standpoint of phonological universals, language acquistion, and pathology just the opposite order of features is inferred. That is, the vowel systems of all languages involve distinctions in height, of most, distinctions in displacement, of some, rounding distinctions, and of a few, nasal/oral contrasts. Vowel distinctions apparently emerge in the child in the same order. (Jakobson, 1968, and Salus and Salus, 1974), and P. Miller (1973) reports at least one case of speech pathology involving a child with only tongue height distinctions for vowels.

This apparent inconsistency between inferences drawn from similarity studies and those drawn from other sources is perhaps resolved if we think of a phonological space as finitely defined by some set of n dimensions. A highest ordered feature in the phonological hierarchy divides this space into m subspaces. Then the next lower ordered feature divides one or more of these m subspaces into p subspaces, and so forth. Similarity judgments involving the highest ordered feature have the entire phonological space in which to be made. But judgments involving each successively lower ordered feature have a smaller subspace in which to be made with the result that similarity judgements become squeezed. So, for example, nasalized vowels are judged to be maximally similar to one another because, by the time the nasal feature applies, the finite vowel space has already been divided into subspaces involving tongue height, tongue displacement, and rounding. The nasalized vowels have therefore been squeezed into a very small space, and are therefore felt to be very similar.

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THE INADEQUACY OF BIPOLARITY AND DISTINCTIVE FEATURES: THE GERMAN "VOICED/VOICELESS" CONSONANTS

Dennis Q. Taylor
University of Illinois at Chicago Circle

Towards the end of the tenth century, the schoolteacher Notker Labio of St. Gall made attempts to orthographically represent the phonetic realization of the German stops as spoken in his region. He wrote p,t,k in place of the usual b,d,g at the beginning of words and syllables following voiceless consonants and at the beginning of sentences. The letters b,d,g were only written following vowels and l,m,n,r. Examples of these spelling changes as a result of Notkers Anlautgesetz are: "ter bruoder - unde des pruoder, tes koldes - unde demo golde" (Waterman, 1972). As Notker perceived them, actual voiced stops only occurred following liquids, nasals and vowels. Some phoneticians believe that the relationships observed by Notker ten centuries ago exist in modern German. Von Essen (1962), for example, proposes: "Nachbarschaft eines stimmlosen Lautes bewirkt durchweg Stimmtonverlust. Steht eine Media im absoluten An- oder Auslaut, so hat sie eben auch stimmlose Nachbarschaft und hat, wenn überhaupt, so nur geringe Stimmhaftigkeit." German mediae in initial and final positions assimilate neighboring voicelessness. Von Essen believes that these relationships have been in effect in German since Notker's day. He does feel that these voiceless-by-assimilation stops are not identical with /p,t,k/. They are voiceless lenes.

Meinhold and Stock (1963) qualify the position of Notker and Von Essen, stating that their experimentation shows a differentiation between b,d,g in absolute initial position and following voiceless consonants. Initial b,d,g are more often voiced. Their figures show that 47.6% "plosions" were completely voiced by their news commentator informants. They also found that initial /g/ was more often voiced than were /b/ and /d/. Other German linguists attempting to specify voiced/voiceless distribution include Schmitt (1947),who combines rules for the distribution of aspirated stops with a fortis/lenis distinction, and Dieth, who gives four allophones for each stop pair, e.g. "tenuis aspirate /p^h/, tenuis fortis /p/, tenuis lenis /b/, and media /b/" (1950). Incomplete as they are, these and other studies of German voiced/voiceless parameters such as Lotzmann's (1958,59) fine investigation of aspiration all indicate that voicing in German is often strongly environmentally effected. It is not a simple matter of presence/absence of vocal cord vibration.

Aspects of voicing have been much more thoroughly studied with consonants in French and English than in German. In the last decade of the past century, for example, L'abbé Rousselot found that the force exerted on an exploratory bulb is greater for surds than for sonants. He proposed that there is decreasing force of articulation

from /t/ to /d/ to /n/. He also called voiceless stops "fort" and voiced ones "faible" (1924). Rousselot concluded from his study that the distinction between air pressures for voiced/voiceless consonants is more consistent and conclusive than the presence/absence of actual voicing. Since these early opinions of this fine pioneer phonetician, the battle has progressed between those who cite force of articulation as a major factor in the voiced/voiceless distinction and those who prefer the more traditional aspect of vocal cord vibration. Stetson (1951) used an airplane guage to measure buccal air pressure and found that pressure is greater for voiceless consonants than for voiced. He concluded that pressure variation is more important than voicing in distinguishing surds from sonants. Malécot has continued the force of articulation investigation in a series of studies (1955, 1966a, 1966b, 1968a, 1968b, 1969, 1970). He feels that "force of articulation is a significant attribute of consonants and enters into the lenis-fortis opposition in such pairs as /p/:/b/ and /s/:/z/."(1970) According to Malécot, force of articulation has little to do with articulatory energy; it is rather a mistaken proprioceptive impression based primarily on intrabuccal air pressures. Perhaps the most vociferous challenges to Malécot's theory come from his former co-workers at Haskins Laboratory, Lisker and Abramson (1964, 1970, 1971).

The position of Lisker and Abramson can best be summarized as follows: "To be sure, many published data do in fact corroborate Stetson (and Malécot) to the extent of showing generally greater pressures for /p,t,k/ than for /b,d,g/, but they demonstrate neither the independence of a fortis-lenis dimension nor its priority over voicing as a distinctive feature for the English stop system"(1970). They feel that their data are "counter to the view of a fortis-lenis difference qua buccal air pressure difference as fundamental to the /p,t,k/:/b,d,g/ contrast." Lisker and Abramson propose regarding their and others research on the subject: "All these data have led us to suppose that it is primarily in their control of the timing of laryngeal adjustments relative to supraglottal gestures, rather than in differences among those supraglottal gestures, that speakers manifest their choice from a set of homorganic stop categories"(1971). Their conclusion is "that voice onset timing is THE SINGLE MOST EFFECTIVE MEASURE whereby homorganic stop categories in languages generally may be distinguished physically and perceptually."

The present study provides additional data for fueling the voiced/voiceless-lax/tense fire. It combines the investigation of supraglottal air pressure variations with that of voice onset/cutoff timing, expanding the former experiments of Malécot and of Lisker-Abramson to include voiced/voiceless consonants, stops and fricatives, in all three positions in connected speech. Since the language under investigation is German, several additional questions are attempted to be answered, e.g. are all final German consonants devoiced? are initial consonants "voiced" in German? etc.

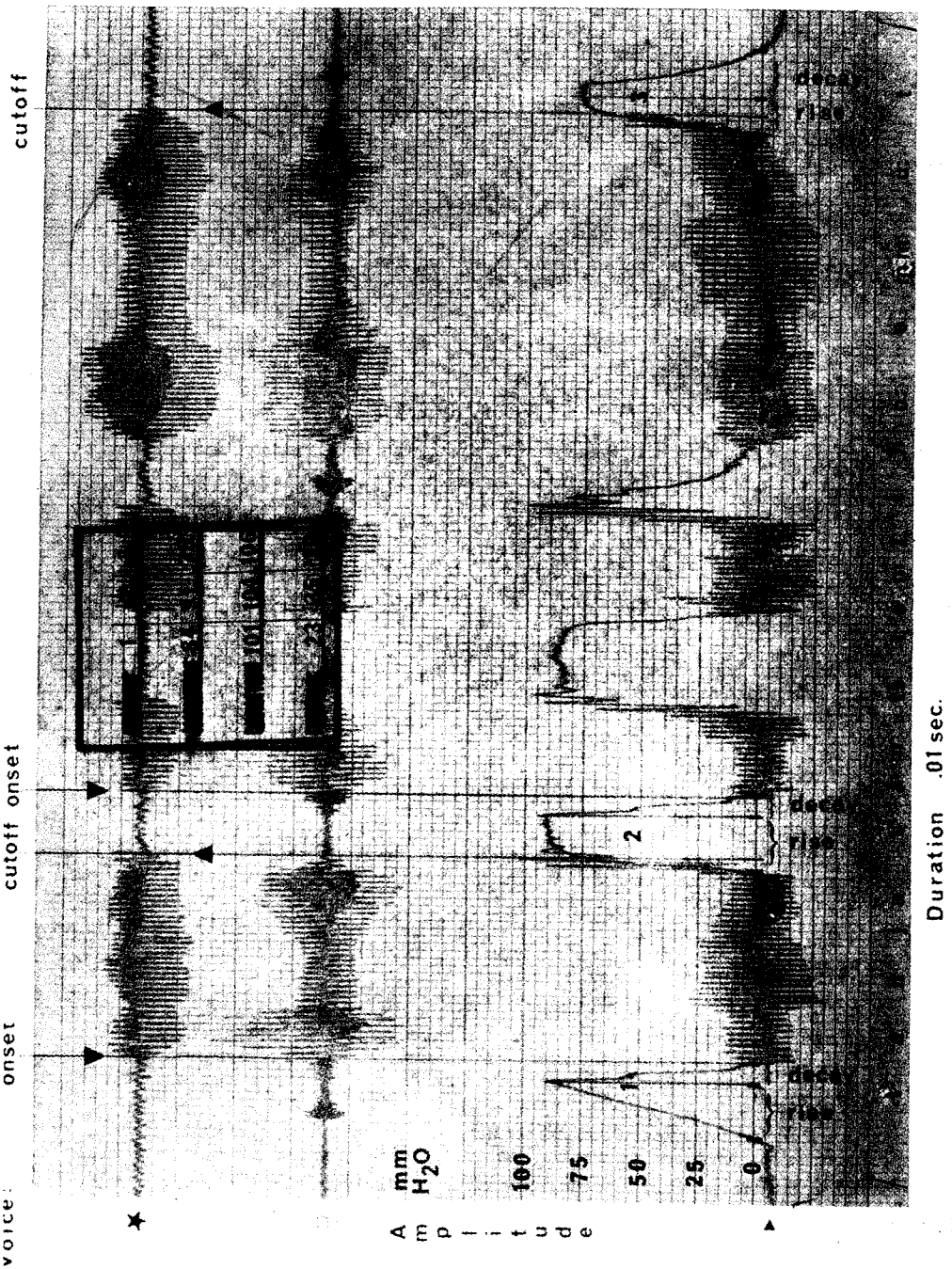
voice onset
cutoff onset
cutoff
Amplitude
mm H_2O
100
75
50
25
0
1
2
decay
rise
Duration .01 sec.
throat microphone
microphone
supra-glottal pressure

FIGURE 1

Methods and Procedures

Figure 1 on the preceding page illustrates the oscillographic read-out and methods of investigation used in the present study. A corpus was created containing German sentences with /p,t,k,b,d,g, f,s,ʃ,v,z,ʒ/ in sentence-initial, medial and final position. Each sentence was octo-syllabic with the consonant under study always occurring in a stressed syllable. As an example of the twelve sentences, the one in Figure 1 is "Tanja tanzte mit hohem Mut." In order to have all twelve phonemes in as many positions as possible, certain liberties had to be taken; two such examples are in the stimuli "Susy Sunshine macht immer Spass" and "Gigi isst den guten Fromage." The language is certainly not Goethean nor even daily conversational, but it is adequately normal for the purposes of this study. Consonants were put in meaningful words and these words into coherent speech. In only one case, final /z/, is an example lacking.

The experimental equipment used in this study consisted of a single-hole rubber catheter inserted via the nasal passage into the mesopharynx of the informant. The catheter is coupled to a small volume pressure transducer which translates the pneumatic pressure variations into a D.C. electrical voltage. This is then amplified by a Honeywell Amplifier System that controls the write-out on a Visicorder light beam oscillograph. The amplifier system and oscillograph form a multi-channel electronic medical system. In addition to the standard microphone, a contact throat microphone was positioned on the neck over one of the quadrilateral plates of the thyroid cartilage in order to provide a more accurate indication of voice onset and cutoff. As seen in Figure 1, the top channel of the oscillographic read out shows the output from the throat microphone; the middle one is from the conventional microphone; and the lowest channel records the output of the pressure transducer.

Various measurements were taken from the oscillograms. These included: 1) consonant duration, measured in milliseconds corresponding to the distance between the point where pressure rises above the ambient level to the point where it returns; 2) peak pressure, measured in millimeters H_2O, corresponding to the amplitude scale to the left of the sample oscillogram in Figure 1; 3) pressure impulse representing the total surface area in mm^2 between the baseline and the pressure curve; 4) voice onset and cutoff as indicated by the vertical lines drawn from the throat microphone oscillogram; and 5) rise and decay times, corresponding to the duration of the pressure buildup and pressure decay.

Ten native speakers of standard German, representing various age groups and lengths of stay in the United States, recorded nine samples of each of the twelve sentence stimuli. Only the five middle samples were measured since primary and secondary series effects had been noted in previous studies.

Results

Table I below contains the results for consonant durations from the ten informants. In the table, mean durations are given in milliseconds with standard deviations and a special percentage representing the mean divided into the standard deviation. This percentage figure allows the reader to directly compare the relative amount of data deviation between various positions and consonants. Three rows of figures are listed for each consonant, representing initial, intervocalic and final positions.

Table I

Mean Durations (in ms)

	St Dev	Avg	Pct		St Dev	Avg	Pct
	11.64	83.06	14.01%		15.19	86.78	17.50%
p	15.77	130.91	12.04%	b	8.20	88.48	9.26%
	16.71	74.26	22.50%		13.34	73.76	18.08%
	16.30	89.34	18.24%		16.46	91.88	17.91%
t	10.77	96.90	11.11%	d	19.13	78.78	24.28%
	21.96	78.46	27.98%		18.23	59.41	30.68%
	10.36	106.26	9.74%		8.45	86.84	9.73%
k	10.89	115.16	9.45%	g	15.57	105.24	14.79%
	20.41	100.88	20.23%		15.99	87.68	18.23%
	23.89	104.52	22.85%		13.57	105.34	12.88%
f	13.12	118.14	11.10%	v	18.84	108.70	17.33%
	18.06	87.04	20.74%		23.47	115.02	20.40%
	18.16	123.48	14.70%		18.25	115.62	15.78%
s	20.82	134.28	15.50%	z	25.39	111.02	22.86%
	21.09	86.72	24.31%		24.36	99.98	24.36%
	15.97	116.36	13.72%		24.86	127.84	19.44%
ʃ	22.32	141.04	15.82%	ʒ	19.18	138.08	13.89%
	33.94	122.38	27.73%		24.04	89.16	26.96%

The data in Table I show that for initial /pb/, /td/, /fv/ and /ʃʒ/ and final /pb/, /fv/, and /sz/, consonant duration is not significantly greater for the voiceless than for the voiced cognates. For all of the consonant pairs in intervocalic position, initial /kg/ and /sz/, and final /td/, /kg/ and /ʃʒ/ there is some duration distinction made.

Table II below lists the mean peak pressure amplitudes in millimeters H_2O. Standard deviations and the previously described percentages are also given.

Table II

Mean Peak Pressures (in mm H_2O)

	St Dev	Avg	Pct		St Dev	Avg	Pct
	11.03	46.78	23.57%		11.00	45.16	24.35%
p	14.75	64.96	22.70%	b	12.90	41.68	30.95%
	13.93	31.62	44.05%		9.73	35.67	27.27%
	17.54	56.52	31.03%		18.74	56.40	33.22%
t	16.17	68.26	23.68%	d	10.40	55.48	18.74%
	13.11	40.34	32.49%		12.00	32.80	36.58%
	13.26	57.24	23.16%		12.21	47.60	25.65%
k	13.91	68.12	20.41%	g	12.82	54.02	23.73%
	8.80	47.13	18.67%		10.87	40.58	26.78%
	17.27	56.22	30.71%		10.33	35.24	29.31%
f	12.71	68.14	18.65%	v	12.16	43.72	27.81%
	10.99	45.86	23.96%		9.26	50.42	18.36%
	13.48	60.98	22.10%		11.09	50.72	21.86%
s	14.21	71.64	19.83%	z	9.84	52.88	18.60%
	7.27	44.12	16.47%		7.18	52.22	13.74%
	15.89	56.40	28.17%		12.82	47.42	26.86%
ʃ	15.11	65.21	23.17%	ʒ	11.75	57.04	20.59%
	6.04	54.42	11.09%		12.20	35.36	34.50%

The mean peak pressures for the ten German informants show that with every cognate pair except initial /pb/ and /td/ and final /pb/ and /fv/ the voiceless member has a greater mean pressure than does the voiced.

Table III on the following page gives the mean pressure impulses with appropriate standard deviations and percentages. Malécot (1970) states that pressure impulse is the most "consistent and powerful" of the three air-pressure pulse parameters: duration, peak pressure and pressure impulse, in correlating with consonantal class. He found greater values for each surd than for its sonant cognate. An analysis of variance showed pressure impulse to be the strongest of the three.

Table III

Mean Pressure Impulses (in mm^2)

	St Dev	Avg	Pct		St Dev	Avg	Pct
	3.89	9.90	39.29%		4.18	10.78	38.77%
p	7.66	32.78	23.36%	b	5.68	13.42	42.32%
	7.06	8.16	86.51%		6.03	10.64	56.67%
	6.96	13.02	53.45%		6.69	15.30	43.72%
t	7.50	26.48	28.32%	d	7.38	16.36	45.11%
	7.13	11.80	60.42%		6.80	8.21	82.82%
	5.25	16.55	31.72%		4.59	12.18	37.68%
k	6.21	29.92	20.75%	g	7.50	21.62	34.69%
	7.19	16.73	42.97%		5.85	12.96	45.13%
	9.82	19.72	49.79%		4.59	9.88	46.45%
f	7.97	31.76	25.09%	v	6.13	15.90	38.55%
	6.72	15.44	43.52%		9.00	21.92	41.05%
	7.88	25.00	31.52%		5.91	18.60	31.77%
s	8.51	37.82	22.50%	z	8.04	21.36	37.64%
	5.53	14.58	37.92%		6.79	19.76	34.36%
	8.03	20.40	39.36%		5.06	17.60	28.75%
ʃ	10.13	37.02	27.36%	ʒ	4.37	24.70	17.69%
	8.47	24.74	34.23%		5.85	9.96	58.73%

Each mean pressure impulse is greater for the voiceless consonant than for its voiced cognate with every phoneme pair in all positions except with initial /pb/ and /td/ and with final /pb/, /fv/ and /sz/. (As noted earlier /sz/ in final position are not contrasted in the stimuli.) Initial /ʃʒ/ are not greatly contrasted. The percentage figures are strikingly higher in many cases with pressure impulses than with durations and peak pressures, indicating greater value deviation and inconsistency. Final consonant percentages are often extremely high; this is not surprising, however, since the realization and release of final elements are many times effected by the mood and emotions of the speaker.

Lisker (1970) suggested that pressure buildup and decay timing serves to distinguish /p,t,k/ from /b,d,g/ in medial and final position. He also noted that relative decay time provides the "best resolution of the two stop categories in initial position." Table IV on the next page lists the average pressure buildup and decay times for our German speakers.

Table IV

Mean Air-Pressure Buildup and Decay (in ms)

	Initial		Intervocalic		Final	
	Rise	Decay	Rise	Decay	Rise	Decay
p	63.22	19.84	91.96	38.95	32.72	41.54
b	69.20	17.58	59.64	28.84	28.42	45.43
t	66.36	22.98	64.55	32.35	34.20	44.26
d	65.90	25.98	62.20	16.58	31.86	27.55
k	68.55	37.71	68.10	37.71	43.73	57.15
g	61.88	24.96	66.68	38.56	41.20	46.48
f	67.90	36.62	59.30	58.84	33.52	53.52
v	68.14	37.20	55.58	53.12	44.68	70.34
s	78.83	44.65	75.90	58.38	30.98	55.74
z	72.82	42.80	58.52	52.50	40.26	59.72
ʃ	75.58	40.78	78.88	62.16	43.52	78.86
ʒ	73.06	54.78	71.72	66.36	45.02	44.14

If Lisker is correct in proposing that relative decay time contrasts initial voiced/voiceless consonants, in German only initial /kg/ are distinguished. The decay values for initial /ʃʒ/ are dramatically reversed. In intervocalic position, /pb/ and /sz/ have contrastive rise times, and /td/ have contrastive decay times. Contrasting decay times are also apparent with final /ʃʒ/, /td/ and /kg/. Decay times are noticeably reversed with final /fv/. All of the relationships seen with air-pressure buildup and decay have been noted with the other pressure parameters under investigation: 1) Initial /kg/ are the most differentiated, initial /ʃʒ/ the least. 2)Intervocalic /pb/, /td/ and /sz/ are more contrasted than are the other three pairs. 3) Final /ʃʒ/, /td/ and /kg/ show cognate differentiation.

The final phase of this study concerns the relative timing of voice onset and cutoff. Figure 2 on the next page graphically shows voice timing relationships for the German stops and fricatives. It is in the form of time-bar graphs representing each consonant in the three positions. The reader should view the graphs as pressure oscillograms with imposed voicing stops and starts. Physiologically, the graph for /p/,for example, represents buccal air pressure buildup following occlusion (solid line), release and pressure decay (broken line), aspiration, voice onset of the following vowel (down arrow), voice cutoff during pressure buildup of intervocalic /p/ (up arrow), voicing starting towards end of decay and lasting into final /p/.

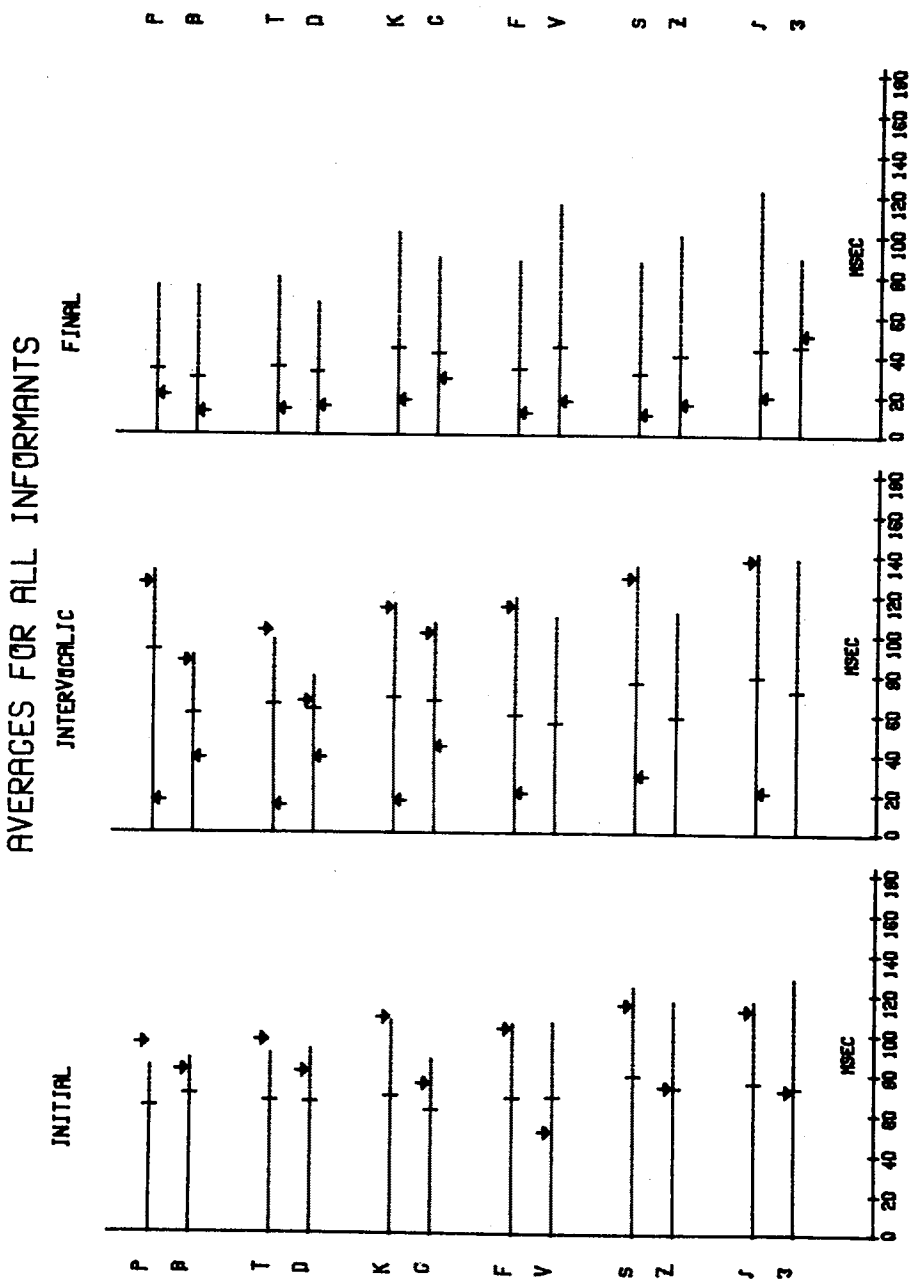
AVERAGES FOR ALL INFORMANTS
INITIAL
INTERVOCALIC
FINAL
P B T D K G F V S Z ʃ ʒ
MSEC
0 20 40 60 80 100 120 140 160 180

FIGURE 2

	Duration	Peak Pressure	Pressure Impulse	Rise Time	Decay Time	Voice Onset	Voice Cutoff
Initial							
/pb/	–	–	–	–	–	+	
/td/	–	–	–	–	–	+	
/kg/	✓	✓	✓	✓	✓	+	
/fv/	–	+	✓	–	–	+	
/sz/	–	✓	✓	–	–	+	
/ʃʒ/	–	–	–	–	–	+	
Intervocalic							
/pb/	+	+	+	+	✓	–	+
/td/	+	✓	✓	–	+	✓	+
/kg/	✓	✓	✓	–	–	✓	+
/fv/	✓	✓	✓	–	–	+	
/sz/	✓	✓	✓	✓	–	+	
/ʃʒ/	–	✓	✓	–	–	+	
Final							
/pb/	–	–	–	–	–		–
/td/	✓	✓	✓	–	✓		–
/kg/	✓	✓	✓	–	✓		✓
/fv/	–	–	–	–	–		✓
/ʃʒ/	✓	+	+	–	+		+

FIGURE 3

Summary of Air-Pressure and Voice-Timing Data

Figure 3 on the preceding page summarizes our results for the German voiced/voiceless consonants. A plus shows marked differentiation, a check some contrast and a minus no distinction. The following points seem valid based on the data presented in this study: 1) In initial position, /pb/ and /td/ are slightly distinguished on the basis of relative voice onset timing. 2)Initial /ʃʒ/ show contrastive voice onset timing only. 3) Initial /kg/ are the most consistently contrasted of any cognate pair. Initial /fv/ and /sz/ have contrasting peak pressures, pressures and voice onsets. 4) Of the cognate pairs in intervocalic position, /pb/ are the most differentiated. 5) The voiced/voiceless members of each medial cognate are contrasted on several bases. 6) Final /pb/ and /fv/ are not distinguished. However, with final /ʃʒ/, /kg/ and /td/ there is some contrast between voiceless/voiced cognates.

Theoretical Discussion

Using the Jakobson-Halle (1968) (Or Jakobson, Fant and Halle, 1965, or Chomsky and Halle, 1968) distinctive feature system, German /p,t,k,f,s,ʃ/ are classified as [+ voiceless] and [+ tense] in contrast to their voiced, lax cognates /b,d,g,v,z,ʒ/. The physical correlates of the feature "voice/voiceless" are given by Jakobson and Halle as "genetically - periodic vibrations of the vocal cords (vs. lack of such vibrations." The feature "tense/lax" is described as "acoustically: longer (vs. reduced) duration of the steady state portion of the sound" and "genetically: a deliberate (vs. rapid) execution of the required gesture resulting in a lastingly stationary articulation; greater deformation of the buccal tract from its neutral, central position; heightened air pressure." (1968) According to these definitions, each German lax/voiced consonant would be characterized by rapid articulation, short duration, lessened air pressure and vocal cord vibration. The results of the present study show that linguistic theory and fancy are not always commensurate with fact and phonetic reality. The two simple features voice/voiceless and tense/lax provide only the merest beginnings of a complete description of the complex system of voicing parameters in German. As a single example consider initial /pb/ and /td/. The stated distinctive features reveal nothing. In our opinion, an additional factor,the presence of aspiration, serves to distinguish between the cognates with little indication of tensity or voicing. Reich proposed that feature analysis is appropriate if "all phonemes with a particular feature behave the same way in particular environments" (1974). The varied behavior of the German "voiced/voiceless" consonants in a given environment would tend to challenge the appropriateness of feature analysis in this case. Ladefoged (1972 and Vennemann-Ladefoged, 1973) pointed out the fact that the difference between English p and b "is not simply that one is voiced and the other voiceless." This and other similar facts "force us into making extremely context-restricted statements for each distinctive feature," he stated. In the case of the German consonants, we also feel that

the number of qualifying statements and rules needed to adequately translate the distinctive features voice/voiceless and tense/lax into accurate descriptions of phonetic reality render the features of little value.

The purpose of this paper is not to propose a new modified system of linguistic description. It is merely to present comprehensive data on the voiced/voiceless consonants in German and to ask the simple question: Why is modern distinctive feature theory still inadequate for describing the existent conditions in German? A fact that seems even more puzzling since Notker Labio, a simple school teacher a thousand years ago, was rather reasonably able to orthographically describe a similar phonetic condition.

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STRUCTURAL REGULARITIES IN THE INTONATION OF AN ABNORMAL SPEAKER: PSYCHOLINGUISTIC IMPLICATIONS

Joseph Oliva and Judith Duchan
State University College at Buffalo

An important characteristic of a limerick or a nursery rhyme is that the syllables fall into canonical stress patterns. For example:

To market # to market # to buy a | fat pig (ᴗ) ||

can be expressed as having a repeated weak-strong-weak stress pattern. English prose, on the other hand, allows for a variety in stress patterning across intonation sequences. Both nursery rhymes and English prose material are comprised of intonation patterns with one and only one primary stress and which are bounded by silence and major juncture, or by two major junctures. These intonation patterns within an utterance tend to be of equal duration. For example, when saying "Okachobee, Florida" the four syllables of the first intonation pattern "Okachobee" are said with about the same duration as the three syllables of the second intonation pattern, "Florida."

What follows is an in-depth structural analysis of the intonation and paralanguage of a fifteen year old abnormal speaker, Joey. The analysis reveals that Joey's utterances are structured in the same way English nursery rhymes are structured, into canonical stress units. These rhythmic stress sequences, while very different in form from units in adult intonation, may be comparable to the intonation structuring of normal two-year-old children. Laura, a normal two-year-old, was videotaped by Witz and Duchan (1973). Witz (1974), as well as Oliva and Duchan (1975), found that nearly all of Laura's longer utterances were comprised of several identical or near-identical rhythmic stress units. For example, her production of "My sissy silly puddy" was made up of a primary and weak stress pattern with <u>my</u> acting as a proclitic (Halliday, 1967).

Methodology

A 100 utterance sample of Joey's language was analyzed in several steps. First, a phonetic transcript was made which included Trager-Smith's (1951) method for notating stress, pitch and juncture. Second, comparisons were made between Joey's intonation and normal adult intonation for the 100 utterances. Third, 44 utterances were selected

from the original 100 on the basis of their length. These were the utterances which were sufficiently long to contain more than one rhythmic sequence. Smith's prosodic analysis (1968) designed for analyzing rhythmic sequences in poetry was applied to these 44 utterances. Smith's analysis differs from the Trager-Smith supra-segmental analysis in that secondary and tertiary stresses are reduced to weak. Finally, a structural analysis of the weak-strong stress sequences was performed on the 44 utterances. The following set of definitional constructs were used to perform the rhythmic stress-based analysis.

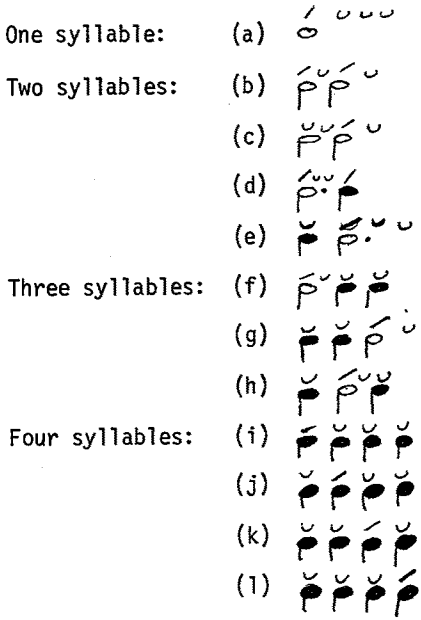
1. Rhythm group - A grouping of weak stress or stresses in relation to one primary stress. Rhythm groups proceed in an ascending direction from weak to primary as in: ∪/, ∪/, ∪∪/ or in a descending direction from primary to weak as in: /∪, /∪∪, /∪∪∪ or, when bounded by terminal junctures, can go from weak to primary to weak as in: ∪/∪, ∪∪/∪, ∪/∪∪, ∪∪/∪∪, ∪/∪∪∪, ∪∪∪/∪ . Rhythm groups can cross single bar junctures, but not terminal junctures.

2. Isorhythmic patterning - Identical repetition of two or more rhythm groups within an utterance. Isorhythmic patterns may or may not be of the same time duration. The repeated rhythmic group which makes up the iso-rhythmic pattern is called the utterance's canonical form.

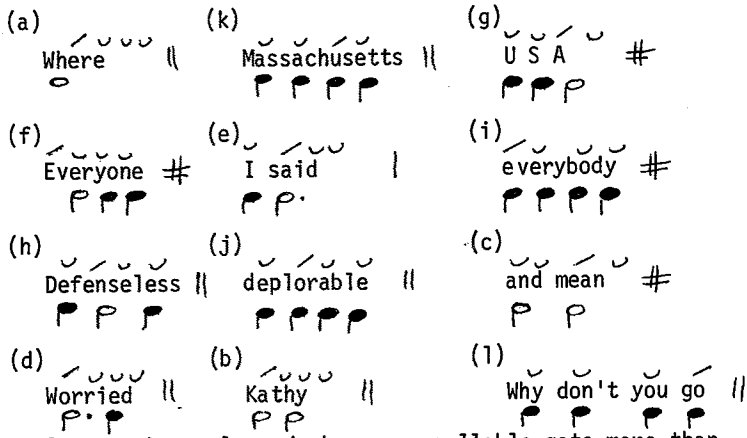
3. Compatible rhythmic pattern - A rhythmic group in an utterance which has the same shape (ascending or descending) as the canonical form of the utterance, but has a different number of expressed syllables.

4. Incompatible rhythmic pattern - An ascending rhythmic group immediately followed by a descending rhythmic group (e.g., ∪/,/∪) or a descending rhythmic group followed by an ascending rhythmic group (e.g., /∪, ∪/).

5. Meter - The number of equally timed beats in an intonation pattern. The meter can be expressed in different rhythmic groups. A four-beat meter, with notes representing syllables and stresses representing beats, can be realized in the following ways:



In verbal context, the above patterns designated by letters could appear as:



Syllables can be prolonged where one syllable gets more than one beat, as in example (g) in the sentence above where "A" can be prolonged to a two-beat count; they can be compressed where two or more syllables get one beat, as in "buckle my"

in the rhyme "One, two, buckle my shoe"; or they can be equally timed as the "one" and "two" in "One, two, buckle my shoe." Pauses are also metric counters. For example, in (g) of the sentence above, the final weak syllable could be expressed as silence rather than drawl.

6. Isometric patterning - Two or more intonation patterns of equal duration. Contiguous isometric patterns can be of the same or different rhythmic groups.

Results

Once the Trager-Smith transcription was done, the comparison between Joey's intonation and adult intonation revealed occurrences of misplaced primary stress as in: "Ĭ hăve tŭ̆o ŭ́nclĕ" or "bў aĭ̆rpláne"; a high incidence of primary stress as in: "Ŭ́nclĕ Jóe lívе Wést Pálm Béach"; and very few occurrences of secondary and tertiary stress. There were an excess of major junctures, and many long paralinguistic pauses within utterances as in "Billy a ride a bike...on the hill." Also evident was a tendency toward drawl, especially on final constituents. Cursory syntactic analysis revealed that articles, auxiliaries, and prepositions were sometimes omitted, and sometimes correctly produced. For example, in "Bill is ride a bike" he produced is correctly, while on another occasion he said "Bill ride a bike", omitting the auxiliary.

While the above description offers a basis for why Joey's speech appeared deviant, it failed to explain inconsistencies or structural systemiticities within Joey's language production. Once systemiticities were discovered in the rhythmic patterning, many of the above inconsistencies became predictable.

Isorhythmicity. The analysis which follows is of Joey's patterning of primary stress, called primary or strong; and all other stresses, called weak (after Smith, 1968). By definition (2) above, two or more identical rhythmic groupings in an utterance were deemed isorhythmic.

All but three of Joey's 44 long utterances were isorhythmic. Each of the three exceptions included place names; two were Okachobee, Florida, and the third was Niagara Falls. In all three cases, it appeared that stress was dictated by the lexical item, for example, ◡◡/◡ for Okachobee, and ◡◡/ for Niagara Falls (said with three syllables). Thus it is suggested that highly rehearsed proper names influenced Joey's production of stress and at the same time interfered with his tendency toward isorhythmic production. These three utterances were among the few in Joey's sample that approached adult rhythmic production.

The 41 isorhythmic utterances were comprised of only six different canonical forms. There were 7 spondee (/), 16 trochee (/∪), 14 iamb (∪/), 2 dactyl (/∪∪), 1 anapest (∪∪/), and 1 anphibrach (∪/∪). Thus, there are by far more two-beat meters than one-beat meters or three-beat meters and, even more remarkable, there were no four-beat isorhythmic sequences.

Of the remaining 41 utterances, 22 were perfectly isorhythmic; 10 were predominantly isorhythmic except for an attached rhythmic group which was usually incompatible with the basic, rhythmic sequence of the utterance; 7 were isorhythmic except for the addition of a compatible weak stress; and 2 were false starts of rhythms which were incompatible with the rest of the utterance and were changed by Joey to become compatible. An example of each of the isorhythmic types will be discussed in turn.

In nearly all of the 22 perfectly isorhythmic utterances, the rhythmic groups were supplied directly from the syllabic units. However, in three of the 22 perfectly isorhythmic utterances, a weak stress was supplied where there was no syllable unit. In one case, it was supplied for a hesitation (p...blood) where the repetition in sound was given a metrical weak stress; in a second case, a vowel was prolonged and the prolongation was given a metrical weak stress; and in a third case, a pause was given a metrical weak stress. These additions of weak stresses were motivated from examining the meter of the utterances. From definition 5 above, a meter has a set number of equally timed beats within each intonation pattern. While this isometric characteristic does not always hold for Joey's utterances, it often does. In the above three examples, the utterances become isometric if the beats of hesitation noise, prolongation and pause are counted within the meter in which they occur. Thus, because the beats seem to be supplied by Joey to achieve time-based isometricity, they were represented in the sample transcription. This made the utterances isorhythmic as well as isometric.

The ten isorhythmic + attachment utterances have special properties in common. The attachment, which can occur either in initial or final position, is separated from the rest of the utterances by a major juncture, and in all but one case is also separated from the utterance by a paralinguistic pause which follows the major juncture. In other words, attachments are, without exception, single rhythmic groups bounded by major junctures. Further, in all but two cases, the rhythmic group of the attachment is incompatible (see definition 4) with the rhythmic groups making up the rest of the utterance. There seem to be two basic phenomena which produced the stress incompatibility of the attached segment. First, there are occurrences of lexical items in the attached constituent which dictates an incompatible stress pattern as in the /∪ of <u>mother</u>, or of <u>bandage</u>. Second, there are the syntactic relations among lexical items in the attached constituent which may, for example, proscribe weak stress to prepositions and articles as in <u>on the hill</u> or <u>on top</u>.

The effect of lexical and syntactic stress on the overall rhythmicity of Joey's utterances can also be evidenced in the seven cases where a single weak stress offers a compatible exception to the isorhythmicity of the utterances. For example, the utterance: Right in | Niagara Falls || New York # is isorhythmic iambic except for the extra weak stress required by Niagara Falls. The same is true for the utterance: Last June # to eight || to thirty # where the additional beat occurs in the final syllable of thirty. Syntactic constraints are exemplified by the requirement of weak stress on the second pronoun I in: I know # I get sun. #

Effects of Isorhythmicity. Joey's language deviates from adult language on all levels. While many of his "errors" must be accounted for by looking at systemiticities at each level, many appear to be reflections of his isorhythmically based intonation system.

At the suprasegmental and paralinguistic levels, it was noted earlier that when Joey's language was compared with adult langugage he was found to exhibit misplaced primaries, to have but few secondary and tertiary stresses, and to have many paralinguistic pauses. Joey's misplaced primary stresses reflect his observance of the canonical form; for example, the dactyl rhythm of Yes I two | uncle # requires that the primary be on uncle rather than on two. The few occurrences of secondary and tertiary stress are also predictable from the apparent binary (primary and weak) restriction of Joey's stress choices. Existence of major junctures and paralinguistic pauses within utterances are often indicators of separations between rhythmic groups or of rhythm shifts which increases their incidence. Finally, drawl serves to give extra beats, and/or establish primary stress which can account for Joey's excessive use of paralinguistic drawl. The features of Joey's intonation and paralanguage are also characteristic of exaggeration in spoken limericks or nursery rhymes.

Syntactically, Joey appears to be inconsistent in his insertion of weak stressed morphemes such as articles and prepositions and in his insertion of deletion of weak stressed syllables. However, in many cases, the rhythm grouping can account for the inconsistencies as is exemplified by the differences between:

Bill ride a bike

Bill is ride a bike

In the former the iambic pattern prohibits the weak stress morpheme, while in the latter the trochaic pattern requires it.

In sum, many of the deviations which were described earlier as features which contributed to Joey's language problem, can be accounted for by considering them as differences which derive from his isorhythmically based intonation system.

Psycholinguistic Implications

Previous studies of normal child language and abnormal language has tended to focus on phonology and syntax, and more recently on semantics, while neglecting intonation. With the exception of Witz (1974) and Halliday (1973), child language researchers have confined their description of intonation in early language to the child's distinction between interrogative and declarative utterances (Menyuk & Bernholtz, 1969; Lewis, 1951) or general emotional tone conveyed by intonation (Tonova-Yampol'skaya, 1973). What is implicit in this form of research is that intonation of the child is highly developed and adult-like, even at the one-word stage of development. Language pathologists, on the other hand, often mention deviation in intonation patterning as a symptom of pathology though there have been no thorough systematic studies of the deviance. (Weiss, 1964; Darley, Aronson, Brown, 1969).

The data presented in this study of Joey, an abnormal speaker, as well as in Witz (1974) and Oliva and Duchan (1975) study of Laura, a normal two-year old speaker, indicate that their intonation patterns not only deviate from adult speakers, but the deviation is highly systematic and relates in systematic ways to other levels of the child's language. The nature of the relationship between intonation and language needs fuller study; however, evidence from Joey and Laura allows us to entertain three possibilities.

1. Language (syntax and phonology) determine the rhythmic groups.

   This hypothesis would, in its extreme, suggest that the stress patterns stem systematically from other levels of language and have no separate psycholinguistic existence. Halliday (1973) found a lack of separation between intonation and segmentals in a 15 month old at the one word stage. At the two-word stage, the pivot-open syntactic constructions may dictate a consistent weak-strong patterning, while the open-open constructions may result in a strong-strong stress pattern. Oliva and Duchan (1975) found trends in this direction from a transcript of Laura at 1-1/2 years.

   A less extreme version of this hypothesis would suggest that sometimes language determines rhythm. Witz (1974) reported Laura at 2-1/2 to have lexically based stress patterns as indicated by her consistent strong weak rhythm of items "daddy" and "Sissy". Similarly, Joey expressed lexically based stress pattern in his production of familiar places (Okachobee, Florida) and common nouns (bandage). Syntactically determined stresses also influenced Joey's production as was seen in his weak stressed production of articles and prepositions.

2. Rhythmic groups influence language. A poet who uses poetic license to fit the language into a metric or rhythmic pattern indicates that rhythm can influence language. Witz (1974) reported that Laura used prolongations as extra beats, and compressed syllables into single beats in keeping with her rhythmic canonical forms. Joey's prolongation of syllables and insertion or omission of weak syllables or function words to conform to the rhythmic group are evidence for the influence of isorhythmicity on his phonology and syntax.

3. Rhythmic groups and language are interactive. The evidence for both intonation influencing language and language influencing intonation would suggest that the two systems are interlocking with either one dominant at different times. The interdynamic functioning of the two systems needs further study.

The results of this study raises a number of psycholinguistic questions. First, do most normal children pass through a developmental stage of rhythmic intonation? If so, how does this patterning become established? How does it influence their language processing? How do they progress from this to adult isometric patterning. Second, what factors determine the establishment of the particular canonical forms in utterances? In Joey and Laura, who have only a few canonical forms to choose from, there is evidence that later stress sequences in the utterance determine earlier ones, and similarly, earlier ones influence later ones. Joey, for example, adds a syllable to "Belmont" to make it closer to the stress of "Massachusetts", and he drawls on syllables in final segments to give them the rhythmic symmetry with earlier patterns. Perhaps a suprafix analysis would indicate that there are dominant sequences in the utterance, and that these dominant sequences might determine the canonical form of the utterance.

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PROBLEMS FROM IMPLICIT PREMISES IN CLASSICAL PHONEMICS: EXAMINATION, IDENTIFICATION, AND CURRENT IMPLICATIONS

Melvin J. Hoffman

State University College at Buffalo

For comparison, I arbitrarily divide time periods, in which modern literature on phonology has appeared into earlier, 1931 to 1960, and contemporary, 1961 to the present. I will use present and past tense when referring to the respective literatures.

Earlier descriptive linguists advanced certain premises concerning the nature and properties of the phoneme which subsequent investigators found untenable. Consequently, many rejected the concept either in whole or in part. This discussion is one among others (Lamb, 1966; Peng, 1975; and Schane, 1971) which advocate retaining some concept of the phoneme but which reexamine and revise both the premises and their applications.

Contrary to implications in some contemporary literature, earlier descriptive phonologists rarely labored under any illusions that their operational definitions and procedures were flawless either in their determining or defending of phone to phoneme assignments. Disagreements over criteria and their application were frequent, and the question of refinement was not ignored. Nonetheless, the large-scale abandonment of the phoneme, and the subsequent siphoning of talent and attention to transformational phonology retarded growth of greater rigor in a number of areas.

A typical topic illustrating such arrested development is phonetic similarity. Listed as one among six criteria determining a class of phones or a phoneme, (Hockett, 1966: 100) defined phonetic similarity succinctly as the sharing of one or more features. (Swadesh, 1966: 35) employed similarity to resolve problems of multiple complementary distribution among phones sharing features. (Twaddell, 1966: 67) claimed phonetic similarity too nebulous and too arbitrarily defined to effect such resolution. Contemporary discussions reiterate or elaborate upon the difficulties or challenge various abuses but provide perspective not solution to the problem of the role and nature of phonetic similarity. (Peng, 1974: 90)

notes that relations and patterning interfere with and complicate judgements involving phonetic similarity. Using "sounds" and "phonetic property"for what others term "allophones" and "feature" respectively, (Hyman, 1975: 64) reports the lack of consensus in earlier literature as to whether allophones merely share a feature or exclusively share a feature.

(Austin, 1957: 538-544) took a tentative step toward greater precision in "Criteria for Phonetic Similarity." He viewed phonetic similarity as a property rather than a determinant of the phoneme. He raised and answered the question whether phonetic similarity had any use. His answer was that it determined the possible limits of allophonic variation. He followed this with arguments supporting his conclusions and with implications both diachronic and synchronic.

Austin's passing prevented his further contribution to this and related lines of inquiry. However, Austin helped point the way to another path, increasingly traveled, now that disaffection and dissatisfaction with Transformational phonology is growing franker and more frequent. The distinction, between what is imprecise and what is invalid, is drawn often, only with great difficulty. Earlier descriptivists tolerated criteria, now considered ill-defined, if the criteria proved useful in operational applications. The predominant reaction to such imprecision has been the rejection of both phoneme determining criteria and the phoneme itself. An alternative reaction such as Austin's was and increasingly is becoming the retention of a revised, refined, phonemic concept. Austin was not the sole linguist attempting refinement but differed from others by approaching refinements on a case-by-case basis rather than on a fit-within-theory or implications-for-general-theory basis.

An area whose refinement has fared somewhat better, as it has been favored with more attention, is non-contrative distribution and the nature of contrast itself. Contrast is complicated by both intra-dialectal and intra-idiolectal variation. Earlier literature implied that if the phoneme were the contrastive unit, sub-phonemic variants or allophones should be out of speaker awareness. Although second language and orthographic errors partially support the contention, contrary evidence exists. For example, I have had few students from Buffalo or Chicago who could not, after a short demonstration, both distinguish and produce both a glottalized and non-glottalized variant of /t/

in words like: "Latin, button, and mountain." Of course, these variants produce no lexical contrasts elsewhere unlike the alternative variants for a word such as "either." (Labov, 1965: 102 fn. 22) finds a speaker's awareness of distinction more dependent upon the social significance of the variants than upon contrast.

Determining contrast through minimal pairs is easily defended, but any introductory text on phonemic field procedures warns that the lexicon does not always provide sufficient opportunities to establish the entire inventory soley by minimal pairs. To complete the inventory, the investigator must rely on less precise and less easily defended considerations of distribution, pattern maintainance, economy, and psychological awareness.

As mentioned earlier, imprecision in definition and application of criteria caused disageements over size and nature of inventories, phonemic status of variants, and desirability of phonemic analysis itself. Conflicting evidence, concerning the relationship of contrast to awareness, would seem, then, merely one more confounding cog in the black box. An alternative to deciding between conflicting evidence, regarding allophonic awareness, is to examine whether there may be more than one type of sub-phonemic variant. Phonological theory of the Aspectual school of linguistics posits three sub-phonemic variants.

(Smith, 1968: 92 fn. 28) retains "allophone" but restricts its use and adds two new terms: "dia-allophone and diaphone." His published definitions reflect that his primary criteria for distinguishing the three types of variants are restrictions in their occurrence. Implications for consideration of distribution and awareness do exist; however, and I propose to present the definitions amplified by the additional considerations.

An allophone is binding upon all speakers of the language and is the sole realization of a phoneme in a given environment. As such, the speaker of the language would not be aware of it as no opportunity exists for comparison with an alternative.

A dia-allophone is binding on all speakers of a dialect but not of the language. The dia-allophone, like the allophone, is the sole realization of a phoneme in a given environment. As such, the speaker of the dialect would ordinarily not be aware of it, but unlike the case of the allophone, the speaker does have an

opportunity to become aware of a dia-allophone if he or she comes into contact with speakers of other dialects who have different dia-allophones in the particular environment. A dia-allophone cannot be distinguished from an allophone unless two or more dialects of a language have been analyzed.

A diaphone is not binding on a speaker of a language, dialect, or even idiolect. It corresponds largely to what earlier literature called a "free variant." A diaphone is not the sole realization of a phoneme in a given environment but is one of a number of non-contrasting alternatives which may be realized in the environment. As such, people may be consciously or subliminally aware of it.

The distinction drawn among the three subphonemic variants offers an answer other than outright rejection to questions about an interrelationship between awareness and distribution among phones in non-contrasting environments.

Alternative diaphones, which non-contrastively occur in the same distribution and which realize the same phoneme, were called "free variants" in earlier literature. (Fischer, 1964: 307-8; and Labov, 1966: 48-50) and others reject the "free" characterization of such variation. This is true not only in phonology but in any aspect of language where prediction, of one among several alternative realizations, does not appear susceptible to explanations drawing solely upon linguistic conditioning. Dialect surveys in Chicago, Detroit, and New York among others have contributed both new understanding of the relation of social and situational environments to variation and quantitative data to support various hypotheses dealing with variation.

The concepts of diaphone and dia-allophone add new dimensions and require new questions from such studies. Can a dia-allophone from one area become a diaphone with positive or negative prestige in another? Are dia-allophones maintained by differing social dialects in the same region or will the dia-allophones of prestige groups frequently be found as diaphones of non-prestige groups? "Postvocalic -r in South Carolina" by (McDavid, 1964) can be re-read profitably with the diaphone and dia-allophone in mind.

Any relationship between distribution and contrast seems suspect since complementary distribution of phonetically similar phones does not guarantee their phonemic identity nor does shared distribution by two

phones guarantee a contrast. (Austin, 1960: 14-18) in "Phonotactics and the Identity Theorem" offered an alternative to abandonment of a contrast-distribution relationship. He shifted perspective as in his article on phonetic similarity cited earlier. Distributional relations cannot be used to determine phonemes. Once determined, however, certain relationships are in evidence. Austin claimed that when you compared the distributional ranges of the allophones realizing any two phonemes, the ranges would neither be totally inclusive nor exclusive. The range of allophonic distribution of any two phonemes would be partially but not totally shared. Using Aspectual terminology, total mutual exclusivity, in distribution of two sets of phonetically similar phones, suggests the possibility of complementary distribution of allophones within a single phoneme or an overdifferentiation in analysis. Paradoxically, when a phone's total distributional range is identical to part or all the range of a different but phonetically similar phone, both may be diaphones of the same phoneme if no contrast is in evidence. The thrust of Austin's two articles was neither total acceptance nor rejection of various criteria. He accepted the criteria not for phonemic determination but as checks for correctness after the determination had already been made. If he were correct, it is still not hard to see how earlier analysts interpreted the criteria otherwise.

I would like to turn to the relation of complementary distribution to contrast. The claim that allophones or dia-allophones are in complementary distribution within a phoneme is not made in ignorance of what authors have said about complementary distribution of phonetically similar phones whose non-contrastive roles have been questioned.

Although the discussions dealt with such concerns as speaker awareness and overlapping membership of the same phone in different phonemes, (Chao, 1966: 48-49) illustrated how often phonological variants which realize a single phonological entity are in complementary distribution. A discussion of phonetic similarity by (Peng, 1974: 90), involving data discussed by Morris Swadesh and W. Freeman Twaddell, also illustrates this.

I deliberately used "phonological variant" and "phonological entity." It is hardly a new idea, but it bears repeating that no reason exists to limit

application of complementary distribution to members of a phoneme. Phonemes too can be in complementary distribution as members of what has been called a morphophoneme. Some confusion in the study of phonology has resulted from not distinguishing between the two types--particularly when the problem of overlapping variants is considered.

I do not propose to review earlier literature on neutralization, biuniqueness, and archiphonemes since these subjects are well covered by Lamb, Hyman, Schane, and Peng cited earlier. I do intend, however, to remark upon certain associations attached to the term "morphophoneme."

For some, the morphophoneme represented merely a convenient cover symbol for both phonologically and morphologically conditioned phonemic alternation. For others, the morphophoneme was autonomous, sharing a reality interdependent with but distinct from its member phonemes. Among supporters of morphophonemic autonomy were those who found the morphophoneme less troublesome and more useful than the phoneme when dealing with diachronic sound change, inter-dialectal phonological representation of morphemes, and the relation of phonological systems to alphabetical orthographies.

It is tempting to eliminate the troublesome phoneme about whose nature and determination so many disagreed and which stood between the phone or feature matrix and the morphophoneme with its advantages. All that remained was the discrediting of the value of contrast.

Yet, not all linguists, transformational or non-transformational, are ready to abandon contrast. (Schane, 1971: 503-504 and 518-519), although rejecting the phoneme as an autonomous level of representation, insists that surface contrast plays a significant role in phonology without which certain diachronic and synchronic effects cannot be explained. Schane agrees that the phoneme is not an intermediate line in a generative derivation but claims it can be determined from surface representations and functions of rules.

(Lamb, 1966: 541-542) agrees with Chomsky and Halle in their criticism of earlier attempts to define rigorous procedures of analysis by criteria like biuniqueness and complementary distribution. Independently concluding what Austin concluded earlier, Lamb views such criteria as properties of a phonemic

solution not as procedural devices, but he finds it of fundamental importance to identify and distinguish features of diversity bearing communicative value from those which are not distinctive.

Lamb's means of incorporating contrast into stratificational theory is too involved for this discussion. Working from a quite different theoretical perspective with quite different hypothetical constructs, Lamb like Schane distinguishes phonological processes which realize morphemes and which are subject to syntactic restraints from phonological processes which permit the analyst to recover contrast. (Fudge, 1969) reaches somewhat different conclusions from related argumentation.

(Smith, 1967 and 1968), however, retains the autonomous classical phoneme. Smith devotes the bulk of his presentation to mechanics and terminology rather than in persuasion or in comparison to other theories. To augment these, I intend to list briefly some of Smith's departures from both contemporary and earlier theories with implications for some familiar problems in phonology. My remarks apply to English as the extent of universal application of particulars is not yet known.

Aspectual phonology retains the concept of phonemic contrast, but not all contrasts are considered equal. Three separate types are distinguished: primary, secondary, and tertiary.

Primary contrasts are found in any dialect by noting restrictions of occurrence which I will not specify here. When the primary contrasts for any dialect are determined, they are slightly in excess, slightly deficient, or exactly equal to the number of minimum lexical contrasts for the language found through inter-dialectal comparisons or what some term: the common core.

Secondary contrasts, mandatory for specific dialects, are not subject to the restrictions mentioned before and are not necessary for the language as a whole. Examples of secondary contrast include: "have" and "halve," "sorry" and "sari," and some speakers' distinction between the noun "can" and the modal "can." Naturally, not all the dialects making these contrasts necessarily use the same phonemes to make the contrast.

Within a dialect or even idiolect, certain phonemic contrasts may or may not occur. The adverb "just" in many dialects sometimes does but sometimes does not contrast with the adjective "just." Such contrasts, of low functional load, are termed tertiary contrasts. Sometimes tertiary contrasts enter through borrowing. Tertiary contrasts for sub-minimal pairs entered some English dialects through French and Russian loans such as "beige, rouge, and Zhukov," so that "beige" may or may not rhyme with "age." Tertiary contrast sheds light on a discussion by (Fries and Pike, 1949) in "Coexistent Phonemic Systems."

Phonemic contrasts must be discussed in Aspectual phonology with reference to the morphophoneme. Where others alter their view of the phoneme, Smith reexamines the morphophoneme. For theoretical reasons and to minimize existing associations with the term morphophoneme, Smith introduces a new term: morphophone.

The morphophone inventory is found by exhausting the primary contrast inventory of all dialects available. The total inventory slightly exceeds a common core inventory. The morphophone retains the advantages mentioned earlier for the morphophoneme.

Like the morphophoneme, the morphophone has phonemes or phoneme combinations as members, called diaphonemic selections. Within a dialect, these can be in complementary distribution while realizing the same morphophone. Not every dialect has the same diaphonemic alternations. Some have multiple expressions for a single morphophone. The morphophone underlying the vowels in "out" and "loud" has one expression in Chicago. In Toronto and Tidewater Virgina, two different diaphonemic selections in complementary distribution express the same morphophone although the particular diaphonemic selections differ for the two different dialects.

Aspectualists hold the classical phonemic concept that each phone realizes or actualizes one and only one phoneme, but a diaphonemic selection may express more than one morphophone--even within an idiolect. Overlapping variation is common.

Those variants from which primary contrasts are drawn, are called pure variants and differ from dialect to dialect while maintaining the necessary contrasts between lexical items. Variants conditioned by various phonological environments are conditioned variants.

Morphophones are not always expressed. The final morphophone in the noun "hymn" is such a case. Not all phonemes express morphophones. The /p/ in some speakers' "warmth" or the /r/ in some speakers' "wash" results from dialect-specific phonotactic requirements. The choice of weak stressed vowels in words like "little" and "bottom" are also taken as both dialect-specific and non-morphophonic.

Many properties attributed to the morphophone are true of the earlier morphophoneme. Aspectual phonology departs from other theories, however, by claiming that morphophones themselves are in alternation with one another and are not necessarily invariant in the same lexical item throughout the language. Three separate morphophone alternations are distinguished.

The first type, substitution, includes inter- and intra-dialectal variation of morphophones within lexical items. Some substitutions are primarily fostered by phonological environments which may condition in one dialect but not in the whole language. Loss of contrast between "pin" and "pen" or "pour" and "poor" results from the substitution of one morphophone for another in the latter alternatives, not from phonemic neutralization. Historical processes may account for alternatives in words like: "creek, roof, room, route" etc. A given dialect may select only one alternative or may maintain alternatives with divergent referents, with divergent syntactic functions, or with social or situational significance. The words "either" and "neither" seem this latter type. Substratum and other influences might account for the unexpected /l/ not /n/ in "chimney" or /i/ for /e/ in "get."

The second type of morphophonic alternation, replacement, concerns what earlier linguists termed morphologically conditioned phonemic alternations within the morphophoneme. Aspectual phonology departs from this position by considering the alternation above the phonemic level. Replacements take place in the environment of paradigmatic and derivational affixes. Assuming lexically and dialectally specific morphophone replacement eliminates proliferation of morphophonemes to account for "wife, wives; sing, sang, sung" etc. Replacements are shared by many dialects but not necessarily all. Some differences are well known: "roofs or rooves; hoofs or hooves." Others are sometimes overlooked because of low dialect prestige: "knowed or knew, helped or holp, climbed or clum." The most well-known difference is in the word "greasy" where the replacement's occurrence or non-occurrence marks an important dialect boundary.

The remaining morphophone alternations are termed alterations. They are, with a few exceptions, binding on all speakers of the language. They are subject to certain environmental restrictions. Unlike most substitutions, alterations may be reflected in orthography. Examples include: "sublime, sublimity; profound, profundity; prophet, prophesy; and music, musician."

Aspectual phonology claims that all phonological variation is systemic whether or not the systems involved be linguistic. That the basic constituents of morphemes may vary inter-dialectally within specific lexical items requires morphophonic alternations. That different dialects express these basic constituents with different diaphonemic selections, with different numbers of selections in different distributions with different points of contrast and neutralization requires phonemic alternations. That the analyst must eventually specify the fine phonetic features requires phone alternations.

The goal of establishing generalizations without a sufficiently sensitive framework can lead to ignoring data by supporting the denial of the necessity to record it. Aspectual theory recognizes limits in predictibility e.g. in "penalize, economic, senile, lever, evolution." etc., the first vocalic morphophone will be one of two alternatives. What alternative a particular dialect or idiolect chooses for each word is another matter. Not all morphophonic expressions are predictable. In some dialects, the vocalic morphophone of the possessive "our" has two possible expressions, but not the noun "hour"; likewise, in some dialects, the vocalic morphophone of the contraction "I'll" has two expressions, but the noun "aisle" has one. The phoneme cannot provide the generalities earlier linguists demanded. Aspectualists contend that earlier linguists conflated the phonemic and morphophonic level. Simplicity criteria, economy of symbolism etc. may play a minor role in contrastive analysis. This characterization as well as the value of precise phonetic specification concluded "Phonemic Overlapping" by (Bloch, 1966: 96) originally published in 1941. Simplicity or generalization can be illusory. (Lamb, 1966: 555-556; and Peng, 1974: 100-111) refute the transformational claim that greater generalizations result from the abandonment of a contrastive unit.

I will present my last set of examples from the Aspectual perspective to illustrate overlapping variation, neutralization, and the role of contrast in phonological restructuring. The examples are familiar to many. The words "marry, merry, and Mary," for a substantial number of dialects, have three different vowel expressions.

In dialects found in Buffalo and Chicago, however, the three have the same. To account for differences between the dialects making the contrast versus the Buffalo dialect, no recourse to morphophonic restructuring is necessary. In the former case, different morphophones have different expressions; in the Buffalo case, the three morphophones share an overlapping long diaphonemic selection and represent a neutralization of contrast among morphophones. In "marry," the selection is a pure variant of the vocalic morphophone. In the other two words, the expressions are phonologically conditioned variants of two different morphophone units--the same units as in the dialect which expresses contrasts. If a change such as shortening occurs for whatever phonological or social conditioning factor, as in Chicago, two items could be restructured morphophonically through the reassignment of the formerly long diaphonemic selections to the short morphophone expressed in all three dialects in "merry" the adjective. The Chicago case would not be a neutralization but an elimination of a morphophonic contrast resulting in the substitution in the lexicon of new morphophone units in place of the original.

This concludes my discussion of earlier and contemporary points of view which illustrate and support the advantages of now largely-discarded taxonomic concepts such as contrast. I argue not only for the restoration of the baby thrown out with the bathwater but also for part of the bathwater to help clean up certain deeply embedded problems in the field.

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ALTERNATIVES TO MATRIX MODELS IN PHONETICS AND PHONOLOGY

David G. Lockwood
Michigan State University

The matrix is the most widely used format for the representation of phonetic and phonological transcriptions in modern theoretical, as opposed to applied, linguistics. This paper is intended to examine some of the drawbacks of this formalism and discuss some possible alternatives.

The MATRIX, as used in phonetics and phonology, may be taken to be a formalism organized along two dimensions. The horizontal dimension represents the sequencing of adjacent segments, while the vertical one divides each segment into an integral number of phonetic or phonological features. At the point where a given row intersects with a column, one finds an indication of a feature value for the feature represented by the row for the segment represented by the column.

In examining the advantages and disadvantages of the matrix formalism relative to other possibilities, it is convenient to view the matrix as founded on three fundamental assumptions:

1) BUNDLING: A phonetic or phonological segment is taken to be a combination of several simultaneous properties.

2) FEATURE-AND-VALUE REPRESENTATION: The various phonetic or phonological properties of different segments are organized into related groups termed FEATURES, and the actual property present for a given segment is specified by assigning one or two (or sometimes more) possible values to the appropriate feature.

3) UNIFORM DOMAIN OF SPECIFICATION: The specification of a property applies for a domain of a single segment only, and a property which continues for several segments must have a separate indication for each segment.

Of these three assumptions, that of BUNDLING appears to be the least controversial. There is nowadays a fairly broad, though less than unanimous, cross-theoretical concensus that segments are best viewed as complexes of simultaneous properties.[1]

There is more controversy, however, about the other two assumptions. The assumption of FEATURE-AND-VALUE REPRESENTATION commits one to what Lockwood 1972 termed a FEATURE SYSTEM, as opposed to a COMPONENT SYSTEM. In a component system, a particular property is taken to be part of a particular segment when the property is present, and when it is not present, no explicit mention of its absence needs to be made. In a feature system, on the other hand, the features are, strictly speaking, a rigidly defined set of classificatory parameters. The values they assume in particular cases sometimes correspond to the presence of a particular property, while other values may correspond to the mere absence of a property, as does the commonly used specification [- Voiced], for example. The secondary issue over the use of binary or multinary features within a feature system will be ignored here.

One obvious basis for objection to feature systems is their requirement that such negative properties be overtly marked in every case.[2] Still another reason to object to the assumption of feature-and-value representation has to do with the distinction between the inventory of elements in a system and the tactic pattern defining the combinatory potentials associated with those elements. With the component view, one has a clearly defined inventory of components with an associated TACTIC PATTERN, whose function it is to define the possible combinations of these elements. With a feature view, on the other hand, one builds some, but far from all, of the tactic restrictions into the feature system itself, since properties assigned as values of the same feature can by definition never be found in the same segment. But many additional restrictions exist to be handled by the use of some sort of tactic pattern in any case. In view of the fact that the feature system by itself cannot ever handle all the pertinent tactic restrictions in a language without the aid of some sort of a tactic pattern, it seems most reasonable to incorporate all such restrictions in a tactic pattern and use a component system rather than a feature system.[3]

In adopting a component view, however, one rejects one of the fundamental assumptions associated with the matrix formalism, and a different formalism is therefore needed. An alternative formalism designed for a component system was suggested a number of years ago by Hockett (1966). He presented a formal mathematical definition of what he termed the STEPMATRIX. The crucial difference between the stepmatrix and the matrix is that the stepmatrix can have different numbers of apparent rows in different columns. In fact, there are no true rows at all in a stepmatrix: there are only columns consisting of one or more unordered components for each segment.

The third assumption, that of UNIFORM DOMAIN OF SPECIFICATION, is shared by both the matrix and the stepmatrix. The reason for controversy over it comes from the fact that it disallows the use of single specifications applying to more than one continuous segment. Such specifications are found in a number of theories of phonology, including the prosodies of the Firthian school[4], and the long

components of Zellig Harris (1944, 1951:125-49). Lockwood 1973 suggested the possibility of arrays resembling the matrix and the stepmatrix, except that they allow for violations of the uniform domain assumption in allowing for PROSODIC ELEMENTS (as they will henceforth be termed). Such arrays were termed the QUASI-MATRIX and the QUASI-STEPMATRIX, respectively.

Figure 1 shows the relation of each of these four formats to the three assumptions discussed above. Figure 2 gives a schematic example of each of the formalisms. An equivalence should be assumed between the example matrix and the quasi-matrix, and also between the stepmatrix and the quasi-stepmatrix, but no further relation is implied.

	Formalisms			
	MATRIX	QUASI-MATRIX	STEP-MATRIX	QUASI-STEP-MATRIX
Assumptions				
BUNDLING	yes	yes	yes	yes
FEATURE & VALUE	yes	yes	no	no
UNIFORM DOMAIN	yes	no	yes	no

Figure 1. Relations of Four Possible Formalisms for Phonetics and Phonology.

a) Matrix

1	1	1	1	1	1
0	0	0	2	2	2
1	2	2	0	0	0
2	1	1	2	2	0
4	1	2	3	3	1
1	2	0	2	1	2

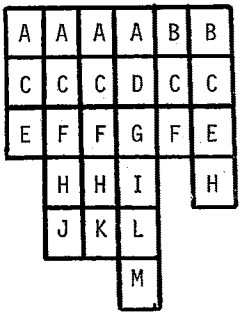
≡

b) Quasi-matrix

1					
0			2		
1	2		0		
2	1		2		0
4	1	2	3		1
1	2	0	2	1	2

Figure 2. Schematic Examples of the Four Formats. (Part I)

c) Stepmatrix



≡

d) Quasi-stepmatrix

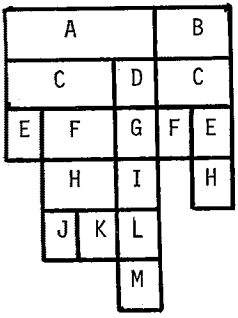


Figure 2. Schematic Examples of the Four Formats. (Part II)

This set of four formalisms far from exhausts the possible alternatives which might be imagined, but for the present just one additional sort of formalism will be mentioned. In order to introduce it, it is necessary to set forth another fundamental assumption which, like that of bundling, is shared by all four formalisms mentioned above:

4) COLUMN-SEGMENT ASSOCIATIATION: Each column is taken to correspond to one of the phonetic or phonological segments being represented, specifying the properties associated with the particular segment in question.

An alternative to the acceptance of this assumption is to set up a formalism in which a column of specifications is associated with the *boundary between adjacent segments*, rather than with the segment as such. In such a formalism, furthermore, one does not specify the presence or absence of a property, but rather the point at which a given activity is *switched on* or *switched off*. The term SWITCHMATRIX provides a convenient designation for a formalism founded on this assumption, and thus on the rejection of the assumption of column-segment association.

Figure 3 illustrates two alternate representations of the phonetics of the Russian word *zdes'* 'here', one using the conventional matrix and the other using one form of switchmatrix. The following points of difference between these two should be noted:

A) In the conventional matrix (3a), each column corresponds to a segment; in the switchmatrix (3b) the columns correspond to segment boundaries, so there are five columns in all instead of four.

a) Matrix

'z,	d,	e	s,	
+	+	+	+	Accented
+	+	+	–	Voiced
+	+	–	+	Palatalized
–	–	+	–	Vocalic
+	–	–	+	Spirant
+	+	–	+	Apical
–	–	+	–	Syllabic
+	–	–	+	Grooved
+	+	–	+	Dental
–	–	+	–	Frontal
–	+	–	–	Closed
–	–	+	–	Mid

b) Switchmatrix

'z,	d,	e	s,	
+	0	0	0	–
+	0	0	–	0
+	0	–	+	–
0	0	+	–	0
+	–	0	+	–
+	0	–	+	–
0	0	+	–	0
+	–	0	+	–
+	0	–	+	–
0	0	+	–	0
0	+	–	0	0
0	0	+	–	0

Figure 3. Russian *zdes'* 'here' Shown Phonetically in Two Formats

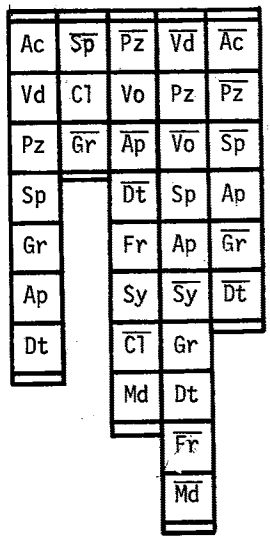
B) The conventional matrix is of a binary type, in which a PLUS indicates the positive value of a feature for a given segment, and a MINUS indicates the negative value; the particular switchmatrix shown is a ternary matrix in which the PLUS value means 'switch on the property', the value ZERO means 'make no change', and the value MINUS means 'switch off the property'.

It should be pointed out that this example illustrates only one possible way of using the switchmatrix format. More extensive investigation of the possible kinds of switchmatrix and their applicability to practical situations of analysis will be needed before we can present any full classification and evaluation of the possible subtypes. Figure 4, for instance, presents a rather different sort of switchmatrix--one more similar to a stepmatrix than to an ordinary matrix. It is actually a direct translation of the switchmatrix of 3b with a two letter component symbol for the positive switching, and the same symbol with a negating bar overlain for the negative switching.

Of the sorts of alternatives surveyed in this paper, the most viable alternatives to the conventional matrix seem to be the quasi-stepmatrix and the switchmatrix. Some of the relative merits of these two formats for use in phonetics and phonology will now be discussed.

In phonology, a fairly good case for the quasi-stepmatrix can be made based on the experience of stratificational phonologists. Since practitioners in this area have accepted the assumptions of bundling and column-segment association, but have used components rather than features and allowed some of these components to dominate sequences of segments, they have in general followed the quasi-stepmatrix format. Examples are provided by Sullivan's work with Russian (1969, 1975),

which uses a voicing component which dominates an obstruent cluster at the bottom of the phonotactics, and stress elements dominating entire syllables. Analogously, tone components dominating entire syllables have been set up for Sherbo of Sierra Leone by Henry Rogers (1967) and Lockwood 1972 suggest the use of word-length prosodies for the handling of vowel harmony (247-50) and syllable-length prosodies to handle the phenomenon of syllable-fronting in Old Russian (261-3). In practice, the quasi-stepmatrix seems to have worked out well for the treatment of these and other phonological problems.



Ac = Accented
Ap = Apical
Cl = Closed
Dt = Dental
Fr = Frontal
Gr = Grooved
Md = Mid
Pz = Palatalized
Sp = Spirant
Sy = Syllabic
Vo = Vocalic
Vd = Voiced

Figure 4. Another Representation of *zdes'* in a Componential Switchmatrix.

In phonetics, however, the case for the quasi-stepmatrix is not nearly so clear. The lack of clarity is in part due to the fact that only preliminary spadework has been doen in formalizing the phonetic level[5], either by stratificationalists or any other modern theorists. Lockwood 1973 suggested that the quasi-stepmatrix might be an appropriate formalism for phonetics, just as it seems to be in phonology. There are problems with the use of this formalism, however, which pertain to phonetics in particular and do not extend to phonology. Such problems are concerned especially with the establishment of an objective, language-universal system of the type generally expected for phonetics, rather than with the more language-specific systems of phonology.

One such problem concerns how one establishes and delimits a prosodic element on the phonetic level. Should a phonetic property

which is shared by adjacent segments always be treated as prosodic? The answer to a related question in phonology is clearly 'no': what is treated as phonologically prosodic in one language need not be so treated in another. Consider, for example, the rounding of vowels and adjacent consonants. Suppose that [$t^{w}um^{w}on^{w}$ɔ] is a typical word in three different languages A, B, and C. In language A, as it turns out, there is a quite general vowel harmony with respect to rounding, which justifies the extraction of a general rounding prosody for the whole word. In language B, however, it is the case that a syllable with a rounded vowel must also begin with a labialized consonant, but there are no general restrictions on the occurrence of such syllables in the same word, justifying separate syllable-level prosodies for each syllable of the word. In language C, finally, no such restrictions exist at all, and it is just a coincidence that each segment of this word happens to be rounded, which would justify no rounding prosodies at all. On the phonetic level, however, it is generally desirable that phonetic sames be treated as the same across languages: it should be possible to establish a single phonetic representation for the word [$t^{w}um^{w}on^{w}$ɔ] regardless of the language it occurs in. Assuming the desirability of such a practice, the question is what sort of principle should be followed to allow objectivity in the phonetic use of prosodies. Within the formalisms accepting the principle of column-segment association--those summarized in Figure 1--the following possibilities seem to exist:

1) Use no prosodies at all, thus remaining with the matrix or the stepmatrix.

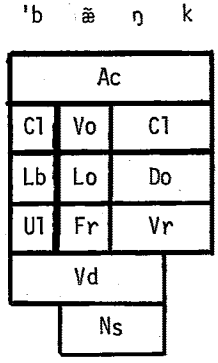
2) Use prosodies as much as possible, treating every case of a property shared by adjacent segments as a prosody.

3) Evolve an objective criterion for deciding when phonetic prosodies should and should not be used.

Combined with the use of the quasi-stepmatrix formalism on the phonological level, the first alternative would have a rather paradoxical result: it would make the purported phonetic representations actually less close to what happens in articulation than the supposedly more abstract phonological representations. It thus seems questionable at best whether this alternative is appropriate.

The second alternative was suggested in Lockwood 1973, but it was nevertheless recognized there that there was a potential problem in fully formalizing cases of prosodies showing a partial overlapping in their domains. The problem is concerned with the use of the stratificational relational network notation, which in general has proven so flexible for various uses. It can readily represent quasi-stepmatrices in general, but it appears to offer no ready means of representing the theoretically imaginable case in which one prosody has segments 1 and 2 as its domain, while another has segments 2 and 3. Preliminary considerations, however, did not reveal a clear case of such an overlap in practice, so it was tentatively concluded that the

phenomenon might not actually occur. More recently, however, it has become clear that such cases do indeed occur, as in the English word *bank*. Here a prosody of voicing could be said to encompass the first three segments, one of nasality is found in the second two segments, and prosodies defining dorso-velar articulation and closure encompass the last two segments. As shown in Figure 5, such words as this give the troublesome overlapping prosodies if the second criterion is followed.



Do = Dorsal
Lb = Labial
Lo = Low
Ns = Nasal
Ul = Upper lip
Vr = Velar

(Cf. also Figure 4)

Figure 5. Illustration of Overlapping Prosodies in the Phonetics of English *bank* (Quasi-stepmatrix).

As for the third alternative, there is at least a strategy by which we might proceed to evolve an objective set of criteria for deciding what is and what is not a phonetic prosody. It involves using the same general principle used in Lockwood 1973 to deal with problems of segmentation and contrast in universal phonetics. The general principle involves making distinctions as finely as necessary to deal with situations in different languages, but no more finely than any known language distinguishes phonologically. Applied to contrast, for example, such a principle requires one to represent a phonetic distinction if there is a corresponding phonemic distinction in any known or possible language. Applied to segmentation, it requires one to segment phonetically at any point at which we need to segment phonemically in any particular language, provided there is a clear PHONETIC basis for segmentation in the first place. The application of this general principle to the use of phonetic prosodies would lead to the treatment of adjacent identical properties as prosodic only if our general analytical principles of phonology would require such situations to be treated prosodically in any language whatever. There are some important unanswered questions, however, regarding the practical effect of the application of such a principle. It appears, on the one hand, that it would severely restrict what is considered phonetically prosodic, requiring no prosodization at all,

for example, in the earlier case of [$t^{w}um^{w}on^{w}$ɔ]. In such a case it might be open to the same objection as the failure to use phonetic prosodies at all. On the other hand, it is by no means clear at this point that a criterion would automatically eliminate all the troublesome cases of overlapping prosodies, thus making it open to the same objections as the second criterion.[6]

This state of affairs makes it seem at least worthwhile to devote more careful consideration to the possibilities offered by the switchmatrix. Even at this early stage of investigation, in fact, it can be demonstrated that a form of switchmatrix can easily handle the troublesome overlapping prosodies and still be susceptible to formalization in a relational network. This point is shown by Figure 6a, which translates Figure 5 into still another type of switchmatrix. This particular form of switchmatrix differs from those in Figures 3b and 4 in that it incorporates two special conventions in order to reduce the number of negative specifications to a minimum. According to one of these, incompatible articulations are shown in such a way that the switching on of one requires that the other be switched off, so the second column shows no -Cl or -Lb, since its +Vo and +Fr are incompatible with such articulations anyway.[7]

a) Switchmatrix

'b	ãæ	ŋ	k	
+Ac				#
+Cl	+Vo	+Cl		
+Lb	+Fr	+Do		
+Ul	-Ul	+Vr		
+Vd			-Vd	
	+Lo	-Lo		
	+Ns		-Ns	

Figure 6. The Treatment of Overlapping Prosodies in the Switchmatrix and Relational Network (Part I)

b) Relational Network

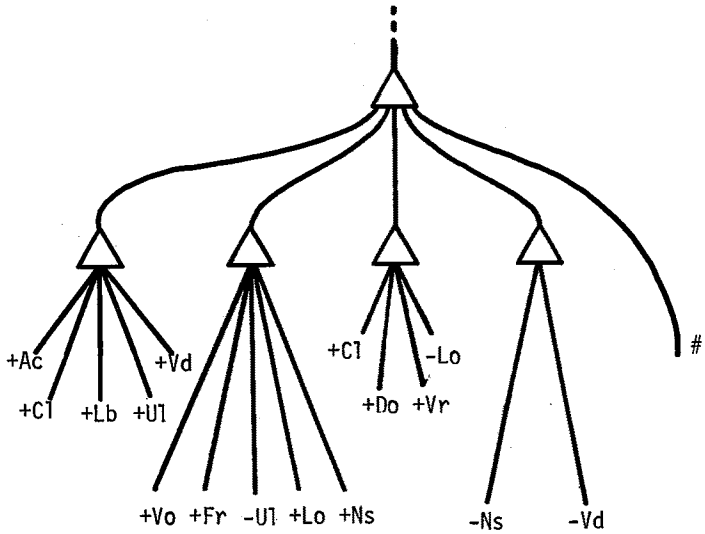


Figure 6. The Treatment of Overlapping Prosodies in the Switchmatrix and Relational Network (Part II)

The double cross in the last column, furthermore, is taken to be an element which turns off all articulatory activity in progress, returning the speech mechanism to ordinary breathing. Figure 6b, furthermore, shows how this switchmatrix may be translated into a network of relationships of the sort used in stratificational linguistics. It should be emphasized that this example is doubtless only a crude approximation in a number of ways. It does, however, illustrate the basic point that a switchmatrix formalization of overlapping prosodies can readily be translated into a network of relationships. This discovery should encourage the more active investigation of switchmatrices by advocates of relational network formalisms in particular.

It seems reasonable to conclude that while the quasi-stepmatrix may remain the preferable format for phonology, the switchmatrix deserves more careful investigation as a model for phonetics. Some papers (Reich 1975, Griffen 1975) have even urged the use of a form of switchmatrix in phonology as well. Any *a priori* assumption that the same format and feature-component system should necessarily be used in both phonetics and phonology seems unwarranted, though one should not assume the converse of this notion either. There seems to be reason to hope, however, that a switchmatrix model in phonetics will be able to overcome the difficulties presented by overlapping prosodies while still allowing a model of phonetics to relate well

to what actually goes on in articulation when our brains send directions to the various muscles. Further research into the application of the switchmatrix in its various forms is therefore in order.

FOOTNOTES

[1]Cf. Pike 1967:294-306 for a view rejecting 'features' as emic units while accepting them as contrastive-identificational properties of segments. Reich's paper in the present volume rejects the notion of simultaneous bundling for what it terms 'sequential onset features'.

[2]The absurdities of such a requirement are perhaps more obvious when one is dealing with semantics rather than phonology, though the same general principle applies to either kind of componential analysis. Suppose, for instance, we were isolating the semantic properties of cats. With a binary feature system we would have to cite not only +Feline, but also such negative (albeit redundant) features as -Canine, -Bovine, -Equine, -Ovine, -Caprine, -Rodent, -Human, and so on.

[3]It might be thought that the distinction between tactic restrictions built into the system as opposed to those requiring separate tactic statements might correspond to the language-particular restrictions as opposed to those which are universal. While a restriction incorporated into a feature system intended to be universal (such as that of Jakobson, Fant, & Halle 1952 or Chomsky and Halle 1968) would necessarily have to be universal, it will still be the case that the remaining restrictions will include both universal and language-specific restrictions.

[4]The most important theoretical papers on this sort of analysis include Firth 1948, Robins 1957, Lyons 1962, and Hill 1966. Palmer 1970 is a useful anthology containing the first two of these as well as a number of exemplifications of the theory applied to various languages in papers originally published between 1951 and 1961.

[5]Throughout this paper, the term PHONETICS should be taken to refer specifically to linguistic phonetics (the study of allophones by linguists) as opposed to physical phonetics.

[6]It should be noted, however, that if the application of the suggested principle did not eliminate the cases of overlapping prosodies from phonetics, it would have to mean that such cases exist in phonology as well. If such were the case in phonology, it would have implications for the use of the quasi-stepmatrix there which would be just as serious as the objections to its use in phonetics.

[7]Such a general convention was suggested by Reich 1975, though the present example does not apply it in the same fashion suggested in

that paper and carried out with reference to English in Reich's paper in the present volume.

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ON THRASHING CLASSICAL PHONEMICS

Sydney M. Lamb and Ralph Vanderslice
Yale University; Yale and University of Victoria

Taketh the fruyt, and lat the chaf be stille.
--Chaucer

One of the two obvious meanings of our title is based on thrash1 "to flog or beat soundly" and refers to the scathing animadversions of Transformational-Generative linguists, especially Chomsky, on "Taxonomic", "Autonomous", "Neo-Bloomfieldian", or, simply, Classical Phonemics. On the other hand, thrash2 or thresh "to separate wheat from its chaff and straw" suggests the approach to phonemic theory we think appropriate. As a corrective to the polemical zeal with which Chomsky and his followers have sought to discredit their phonological forebears by stringing together real and alleged lapses and inconsistencies, we have undertaken a reexamination of the classical phonemic principles--enunciated or implicit in practice--in the spirit of what Anatol Rapoport (1960) calls "empathetic understanding". What we find is that the recipient of Chomsky's exercises in thrashing1 is merely an effigy stuffed with a by-product of thrashing2: viz, a straw man.

The repetition here of points made almost a decade ago in Lamb's "Prolegomena to a theory of phonology" (1966) is justified by the evidence in Chomsky 1967 that what Lamb said was not understood, and by various indications that Chomsky is sidely believed to have successfully discredited the classical phoneme.

This paper defends the classical phoneme against Chomsky's largely misguided attacks. It should not be taken as a thorough endorsement of classical phonemics, whose real defects--ironically overlooked for the most part by Chomsky--are treated in Lamb 1966. A fortiori this paper should not be taken as an exposition of stratificational phonology.

The most concerted attack on classical phonemics is in Chomsky's Current Issues in Linguistic Theory (1964). There he claims that classical phonemics can be characterized in terms of four principles: (i) linearity, (ii) invariance, (iii) biuniqueness, and (iv) local determinacy. We do not know why Chomsky believes that his four conditions "are ... quite generally accepted", especially as he concedes that the fourth is vacuous if the first two are adhered to; nor how he reasons that "they do follow from familiar definitions of the phoneme" (VBM 409). Linearity was not followed by real linguists except with respect to consonants, where it led to uneconomical solutions (as of the Russian obstruents--see below). To the extent it was followed, we have a valid criticism of classical phonemics, but it is noteworthy that Chomsky and Halle also accept a linearity condition in their analysis of Russian.

Local determinacy is not an independent principle as Chomsky would have it, but part of the linguistic--as opposed to the mathematical--definition of biuniqueness: phones must be uniquely assignable to phonemes if their immediate phonetic environments are known, and phonemes must uniquely (up to free variation) predict phones if their immediate phonemic environments are known.

Invariance and a related condition not identified by Chomsky, that of nonsuspendability (Lockwood 1972) are two unmotivated extensions of biuniqueness encapsulated in the pithy-sounding but ambiguous slogan "once a phoneme, always a phoneme." Among their effects is to bar partial phonemic overlapping, according to which the same phone-type occurring in different local environments can be assigned to different phonemes. Some classical phonemicists accepted invariance and nonsuspendability during some portions of their careers, giving rise to what Floyd Lounsbury (personal communication) calls "Calvinist phonemics". Bloch 1941 clearly accepted partial phonemic overlapping, but Bloch 1950 adopted an extreme form of invariance:

> Each phoneme is defined by the quality or combination of qualities present in all its members and absent from all other phones of the dialect; and every phone that contains such a quality or combination belongs to the phoneme which is defined by it. (RIL 313)

It is interesting to compare Bloch's lucid statement of this superfluous principle with Chomsky's wording, which states

> ... that each phoneme P has associated with it a certain set $\varphi(P)$ of <u>defining features</u> (that is, $P=Q$ if and only if $\varphi(P)=\varphi(Q)$) and that wherever P occurs in a phonemic representation, there is an associated occurrence of $\varphi(P)$ in the corresponding phonetic representation. (VBM 408)

The expression in parenthesis is a pleonasm--a pretentious way of saying that the defining features <u>are</u> defining features--and the whole illustrates Chomsky's penchant for formularizing a plain-prose thought, by no means always accurately, in pompous pseudo-math-ese. It would be nice if greater precision were automatically achieved by couching a concept in mathematical jargon; unfortunately it's an illusion. Yet many linguists nowadays, even some with mathematical training, seem to believe that statements like the above really are more precise than those written in plain English or other natural language.

We agree with Chomsky that linearity and invariance are untenable (though we fail to see this as an impugnment of classical phonemics), we agree that a correct theory must allow for phonemic overlapping, and we agree that the outlook for mechanical discovery procedures in linguistics is dim. On the other hand, we disagree with him on the meaning of "separation of levels", on the use and usefulness of complementary distribution, and--most of all--on his claim to have confuted the sine qua non of classical phonemics, the biuniqueness condition, and the fundamental tenet underlying it, the distinctive-

ness principle.

The invariance principle being incompatible with phonemic overlapping, Chomsky correctly cites an instance of the latter to show the untenability of the former. Some pages later he uses it as an argument against complementary distribution. The example is from Bloch 1941, involving a dialect where apical taps occur both as the allophone of /t/ in Betty, butter, kitty, etc., and as the allophone of /r/ after /θ/ as in three, throw, etc. Under invariance, since Betty contrasts with berry (where the retroflex, intervocalic allophone [r] of /r/ occurs), every phone of the tap type [D] would have to be assigned to the phoneme /t/, giving the unsatisfactory analyses /θtiy/ and /θtow/ for three and throw. This shows clearly why invariance must be rejected and phonemic overlapping accepted. But what about complementary distribution? Chomsky concedes that the solution of Table 1--which is the one Bloch 1941 accepted--is "the only reasonable one" and that it is biunique--

(Table 1)

	θ_V	V_V
/t/		D
/r/	D	r

--but, he says, it "is inconsistent with the principle of complementary distribution. In fact, the allophones [D] and [r] of /r/ are not in complementary distribution since they both occur in the context [be-iy] ('Betty', 'berry')" (VBM 414).

But this is simply not so. The tapped allophone of /r/ does not occur in the intervocalic context. Only the retracted allophone of /r/ occurs in this context. The tapped allophone of /t/, not of /r/, occurs here. Chomsky has been led into a logical blunder by his own imprecise use of terms: a phone-type is not an allophone except as it has been assigned to one or more phonemes, and the two allophones of /r/ are in perfect complementary distribution in Table 1.

The problem Chomsky wants to pose exists only for Calvinist phonemics, but even from that viewpoint his statement is wrong because it doesn't start from raw phonetics. If an invariance condition is wedded to complementary distribution, ruling out phonemic overlapping, then of course we would have to say that "the tapped and retracted phone-types [D] and [r] are not in complementary distribution because they contrast intervocalically." This is correct but irrelevant; far from impugning the principle of complementary distribution per se, it simply restates the untenability of the invariance principle, and of the discovery-procedure approach generally. Thus the principle of complementary--or more correctly, in general--noncontrastive distribution, purged of contamination by invariance and discovery-procedurism, emerges unscathed by Chomsky's assault.

Chomsky's biggest artillery against biuniqueness is Halle's well-known example of the Russian obstruents. The traditional treatment (cf. Figure 1) sets up the members of pairs like [t/d], [k/g], [š/ž] as separate phonemes because they contrast in some positions, but in the case of [c/ʒ], [č/ǯ], [x/ɣ], where voicing is always nondistinctive, each pair would be a single phoneme with voiced and voiceless allophones. This treatment misses the desired generalization in that the voicing similitude, though equally predictable in Figure 1-a and 1-c, has to be analyzed as phonemic in 1-a, and assigned by a morphophonemic rule, whereas in 1-c it is allophonic and assigned by a subphonemic rule. The objection, of course, is not to the ordered layering of rules, but to the redundancy of having the same rule, with the same conditioning environment, twice. Superficially, then, the problem does appear to result from the biuniqueness condition. In fact, however, this phonemicization fails in generality not because it is biunique, but because despite meeting the biuniqueness condition it does not satisfy the more stringent distinctiveness principle.

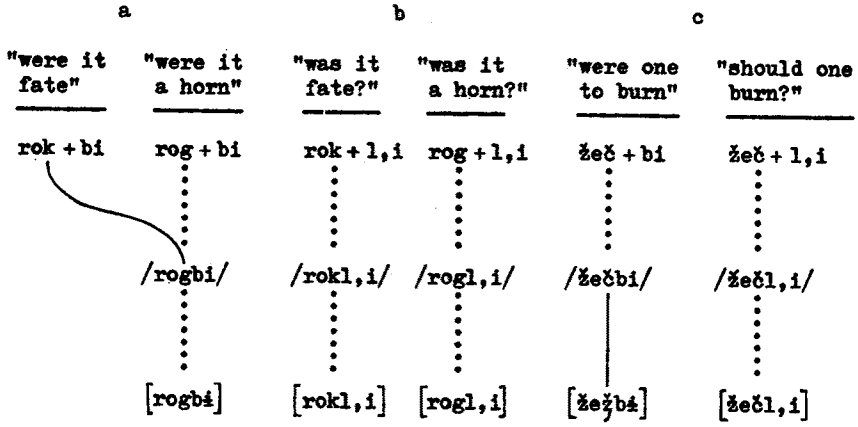

Figure 1. Morphophonemic (a) and subphonemic (c) voicing similitudes

We could, still without violating biuniqueness, capture the desired generalization (in Figure 1) by changing one segmental symbol at the phonemic level. If we were to analyze phonetic [žeǯbi] as /žeǯbi/ (instead of /žečbi/--while of course retaining /žečl,i/), then a single morphophonemic rule would take care of both (i.e. all) automatic voicing alternations, and our quasi-phonemic representations would still be biuniquely related (indeed superficially identical) to the phonetic ones. The only objection is that this "solution" is over-differentiated. Voicing of the phonetic pair [č/ǯ] is nondistinctive throughout the language, so it would flout classical canons to phonemicize both members. Over-differentiation violates, however, not biuniqueness (which bars under-differentiation and neutralization),

but the distinctiveness principle (which bars all three types of analytical error). Note that this new quasi-phonemic level is precisely Chomsky's systematic phonetics--a very broad phonetics with only sporadic over-differentiations keeping it from complete identity with the classical phonemic level.

Chomsky's way out of this dilemma, which explains his offering it as an argument against biuniqueness, is not to phonemicize the phonetic alternation [ǯ~č], but to substitute, as the only linguistically significant level of representation above the phonetics, the abstract morphophonemic representations where rok "fate" and rog "horn" are kept apart even though they are actually homophones (i.e. both are /rok/ in isolation). One has then a "systematic phonetic" level which is almost, but not quite, distinctive (i.e. phonemic in the classical sense), and a "systematic phonemic" level which is nonbiunique and in general almost, but not quite, exactly like the classical morphophonemic one.

What is desired is an analysis that can handle these voicing similitudes in a single rule, or a set of nonredundant rules, and at the same time be not only biunique, unlike Halle's and Chomsky's, but fully in keeping with the distinctiveness principle, unlike the traditional treatment. This can be done within the framework of classical phonemics, assuming the grain (biuniqueness) to be separated from the chaff (linearity, etc.) by the appropriate sort of thrashing.

The traditional treatment of the obstruents, though not obviously an instance of over-differentiation, is distinctive only on a strictly segmental basis. It is comparable to setting up twenty vowel phonemes for a language having five vowel qualities and four tones. In the case of tones and vowels, classical phonemicists accepted suprasegmental, nonlinear analyses as a matter of course. But with voicing in consonant clusters, a strict and linear segmentalism has been the custom--even the Halle-Chomsky treatment has not escaped. There is, however, no principled reason for this difference. Such phonemicizations as the /g/ in /rogbi/ (versus the /k/ in /rok/) violate distinctiveness because they record the presence or absence of voicing in a segment where it is predictable from the immediate environment.

As Johns 1969 observes, "the voicing assimilation is always phonetic, since voicing is never distinctive in obstruents in the position before another obstruent. In other words," he says, "if we have a way of representing the concept of neutralization, which is surely not incompatible with the notion of a phonemic level, Halle's problem becomes totally spurious" (VBM 549). We believe that classical phonemics, prudently interpreted, comprehends the means of accomplishing this effect. We propose, again following Lamb 1966, that the voicing component in all obstruents be stripped off and treated as a suprasegmental phoneme, just as the pitch feature is stripped away from vowels in a tone language. Lamb 1966 used a devoicing phoneme, symbolized /h/; we now regard voicelessness as the unmarked state for obstruents, and we use a voicing phoneme, symbo-

lized /ᵛ/. Thus the voiced cognates of /p, t, k/ are /pᵛ, tᵛ, kᵛ/--phonetically [b, d, g]. The voicing phoneme is a long component, or prosody, whose domain extends across obstruent clusters, so that its occurrence in them is symbolized only once: /kpᵛ/ is the realizate of [gb], etc. (see Figure 2).

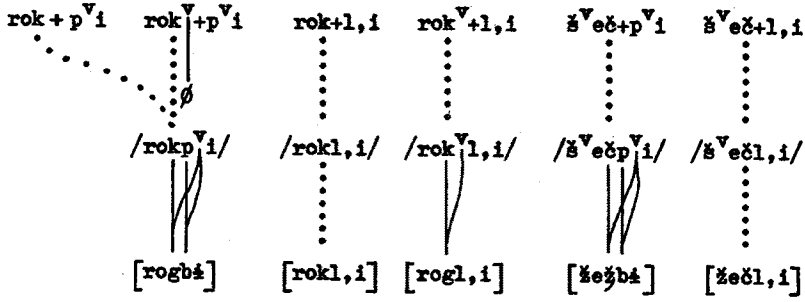

Figure 2. Revised classical phonemic solution of Russian obstruents

This more efficient phonemicization will take care of all the automatic voicing alternations automatically, provided that the appropriate morphophonemic forms--e.g. #rok+pᵛi# and #rokᵛ+pᵛi# --are neutralized at the phonemic level. This in turn is handled by a single morphophonemic rule, viz: "The voicing morphophoneme #ᵛ# is realized phonemically as zero (i.e. has no phonemic realization) before an obstruent or a word boundary; elsewhere it is realized as the voicing phoneme /ᵛ/."

Even by the crude, phoneme-inventory-counting metric of economy, this new analysis is more economical than the traditional one. "In fact it is more economical simply because the distinctiveness principle has been more thoroughly applied than in the case of the former analysis" (Lamb 1966; VBM 614). Indeed, the number of obstruent phonemes in the new treatment is virtually halved, compared to the old, at a cost of adding one voicing phoneme. Lamb observes that

> ... with this more efficient C-phonemicization, motivated independently of morphophonemic considerations, Halle's argument no longer applies. ...
>
> Thus the inefficiency which Halle correctly finds in the traditional phonemic analysis of Russian has nothing to do with biuniqueness. It is the result not of a basic defect in the C-phonemic principles, but of an incomplete and therefore inadequate separation of the nondistinctive features from the distinctive ones.

Lamb continues:

> It is therefore hard to understand why Chomsky responded to this argument by asserting that my position agrees with Halle's! Evidently it is necessary to point out that there is a difference between (1) rejecting a specific analysis for a particular language and (2) rejecting a general principle of phonological analysis. (VBM 614)

Yet, as if this had never been written, Chomsky (1967) again asserts that

> Lamb's attempt to refute Halle's argument merely amounts to accepting it in toto, with a change of notation. But, while accepting Halle's specific analysis of Russian (in effect), Lamb objects that it does not refute "classical phonemics" ... . However, ... to impute the distinctiveness principle in Lamb's sense to classical phonemics is less than just, since much classical work was specifically directed to overcoming the absurdities to which this principle leads. Therefore, we must conclude that Lamb has not met Halle's argument, but has merely accepted Halle's analysis and proposed an untenable principle to justify it, all of this having nothing to do with the issue that Halle raises against classical phonemics. (VBM 537n; emphasis ours)

We do not regard this as a contribution to rational debate. That Halle intended the Russian example to show that the biuniqueness requirement of classical phonemics was accountable for the uneconomical treatment is plain from the published literature. That Lamb does not accept Halle's "specific analysis"--"in effect" or in any other way--has been stated with sufficient force and frequency. The only two of Chomsky's allegations which could conceivably merit further comment are (1) that Lamb's and Halle's treatments of Russian are substantially identical, and (2) that the distinctiveness principle as formulated by Lamb is untenable and leads to absurdities.

(Table 2) ATTRIBUTE	Halle's	Lamb's
Departs from traditional phonemicization	YES	YES
Handles voicing similitudes without redundancy of rules	YES	YES
Preserves linearity	YES	NO
Preserves biuniqueness	NO	YES
Incorporates explicit phonemic level between morphophonemic and phonetic	NO	YES
Employs suprasegmental phoneme of voicing	NO	YES
Conforms to classical phonemic principles	NO	YES

Let us look at the salient attributes of the two treatments of Russian obstruents and compare. Table 2 seems to us to exhaust the significant similarities and merely to sample the potentially criterial differences; in 71% of these attributes the two solutions differ.

Lamb 1966 defines the distinctiveness principle as follows:

> ... a correct C-phonemic solution treats two units (i.e. segments, syllables, or the like) as phonemically different if and only if there is a distinctive phonetic difference between them. By distinctive phonetic difference I mean a difference in at least one distinctive phonetic feature; a phonetic feature is distinctive if its presence is not determined by its immediate environment. (A more precise specification is given below.) (VBM 611)

This is quoted in part by Chomsky (1967), who then characteristically attempts to formularize it:

> Putting it concisely, then, the "distinctiveness principle" asserts that:
>
> (11) Units X and Y are phonemically different if and only if there is a phonetic feature F such that:
> (a) X and Y differ with respect to F, and
> (b) F is not determined by its immediate environment. (VBM 536)

Chomsky then spends two columns refuting this formularization, concluding:

> It seems pointless to pursue discussion of (11) any further. In fact, the "distinctiveness principle," as formulated by Lamb, is nothing more than a vague expression of the intuition that led early structural linguistics to search for viable principles of phonemic analysis that would overcome such absurdities as those just noted--principles which, it should be emphasized, were produced by Jacobson, Bloch, Harris, Hockett, and many other investigators in the course of the careful and serious attempts to develop a significant theory. Lamb's new theory is simply a return to the period that precedes the development of structural linguistics. (VBM 536)

But what Chomsky's (11) gains in concision (about nine words, or 20%) it more than loses in precision. To be sure, Chomsky has anticipated and attempted to forestall the criticism that he "did not express accurately what was intended":

> This principle [distinctiveness] ... is sufficiently vague and inexplicit that an analysis of it must "distort" in the direction of further precision; any such interpretation is, surely, questionable, but unavoidable if these issues are to be discussed at all. (VBM 537n)

Since, however, version (11) as stated is ambiguous, and on its most obvious interpretation (that followed by Chomsky) seriously misrepresents the distinctiveness principle, we are underwhelmed by the vaunted advantages of letter variables and outline form. As for concision, we offer the following correct paraphrase in fewer words than (11): In a correct classical-phonemic solution, "two units (i.e. segments, syllables, or the like) are phonemically different if and only if there is, between them, a difference in at least one phonetic feature whose presence is not determined by its immediate environment."

It may not be immediately apparent that this says something quite different than version (11). But Chomsky interprets clause (a) as meaning that two units cannot be phonemically different unless they differ with respect to the presence of some phonetic feature F. This at once excludes phonemic overlapping--since, obviously, where a single phone type comprises allophones of two phonemes, the two units cannot differ by any phonetic feature--so that (11) is indeed untenable from the outset. Chomsky then refutes it by rehashing his cliché cases, like the Betty-throw problem: "Let X be the [D] of three and Y be the [D] of butter. Since they differ in no feature F not determined by the environment, they are phonemically identical by virtue of the distinctiveness priciple. Evidently the same absurdity will arise in every case of partial overlap" (VBM 536).

Precisely so--for (11) on its more transparent interpretation. But when we say that two units must differ in "at least one phonetic feature whose presence is not determined by its immediate environment", the relative clause is not merely an appositive, nor an anded afterthought as (11b), it is a defining clause. Whether there is a phonetic difference or not is irrelevant; what counts is a distinctive phonetic difference. Phonemically overlapping units, by definition, share all their phonetic features. But if feature "F" is distinctive in "X" and determined in "Y", then X and Y show a distinctive phonetic difference. Phonemic overlapping is, thus, allowed, and Chomsky's examples of it are beside the point.[1]

---

[1] A correct version of the distinctiveness principle in the style of (11) can be given as follows:

(11') Units X and Y are phonemically different IFF there is a phonetic feature F such that:

(a) X and Y differ with respect to either the presence or distinctiveness of F, where

(i) F is distinctive if it is not determined by the immediate environment.

This is wordy because, following Chomsky, it is embroiled with raw phonetic features instead of going directly to the notion of distinctive phonetic features. Chomsky's (11) is ambiguous in that clause

In the whole of Chomsky's tirade against "taxonomic" phonemics, not a single one of his arguments really bears on complementary distribution or biuniqueness; in every case there is covertly incorporated some other offending principle such as linearity, invariance, nonsuspendability, or discovery-procedurism. These are the chaff of classical phonemics, not the grain. We submit that the thrashing appropriate to this body of knowledge, which still offers many insights, is of the winnowing, not the flogging, variety.

References

Note: page numbers are keyed to the following anthologies:

RIL = Joos, Martin (ed.), Readings in Linguistics (Washington: ACLS, 1957; reissued as Readings in Linguistics I, Chicago: University of Chicago Press).

VBM = Makkai, Valerie Becker (ed.), Phonological Theory: Evolution and Current Practice (New York: Holt, Rinehart and Winston, 1972).

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(a) can be taken (as Chomsky takes it) in its obvious meaning "X and Y differ with respect to the presence of F," or it can be interpreted as synonymous with (11'a). These facts reconfirm our belief that formularism is no substitute for attention to content, and that failure to interpret one's opponents' expressions with empathetic understanding leads to gamesmanlike rather than rational debating.

ABSTRACTNESS, THE SYLLABLE, AND THE FLEETING VOWEL IN RUSSIAN

William J. Sullivan
University of Florida

0. Introduction

The syllable--vox articulata--is a maltreated and badly misunderstood stepchild of current phonological theory. There are reasons for this apart from the practical difficulties of accounting for all the possibilities in a given language. For example, almost all the objective data one can gather from acoustic phonetics seems to indicate that the syllable can not be defined on any mechanically simple basis. Nor, apparently, can it be given an abstract universal definition. But the universality of the occurrence of syllables in spoken language has been widely recognized, and the utility of the syllable in language description has not gone unnoticed. Hockett (1947) described the phonology of Peking Chinese on the basis of the syllable, and Bernard Bloch used to give his beginning linguistics students a rule-of-thumb definition of the syllable as "the minimum repeatable stretch of sound" (vox articulata?). Haugen (1956:215) developed a methodology for describing the syllable structure of a particular language and defined the syllable in terms of "the development of the phonotactics, or the study of phoneme distribution ... as the *smallest unit of recurrent phonemic sequences* (emphasis in original)."

Halle (1959 and 1962), defining generative phonological theory, attacked the notion of a biunique segmental phoneme, presumably administering the coup de grace to neo-Bloomfieldian phonology (but cf. Johns 1969, Peng 1975a and 1975b, and Sullivan 1975). Since Haugen's syllable was defined in terms of segmental phonemes, it also went by the boards. (But judge the (un)fairness of this extrapolation by re-reading Hockett 1947 and see what he had to say about features, segments, and the syllable.) Be that as it may, Halle's formulation of the generative phonological model did not include the syllable. With few exceptions, most notably Saporta & Contreras 1962, generative phonologists ignored it. Sampson 1970 demonstrated, however, that a non-segmental syllabic base can provide for generative phonology a non-ad hoc solution to the question of determining underlying forms for languages without morphophonemic alternation, e.g. Vietnamese.[1] Hooper 1972, apparently in ignorance of Sampson 1970, suggested the application of syllable boundaries to evoke (or block) the application of certain Prules in phonology. I have not seen it as of this writing, but I am told her dissertation includes a definition of Spanish syllable structure. However, it remains generally true that the generative model (though not every generative linguist) is incapable of handling the syllable.

But the treatment of alternation is central to generative phonology. Sampson's article is therefore generally ignored by generativists as being off the main issue. Within treatments of alternation, however, there has been a growing split between the advocates of a more abstract approach, as in Chomsky & Halle 1968, and the advocates of a more "concrete" phonology, as in Vennemann 1971 and Harms 1973. Chomsky & Halle 1968 take off on what seem to be some wild flights of fancy. But once the door is opened, as Lightner (1971:546) correctly remarks, "Where does one stop? And why?" Kiparsky has proposed an alternation condition and Vennemann has proposed 'naturalness' conditions in an attempt to limit the amount of rewriting that can be done. But Lightner's inference that such a priori conditions are more-or-less ad hoc articles of faith is still valid. There is no way of testing their metatheoretic significance in the general case. Jensen 1974 has shown in a stimulating article that there are actually 9 different and non-equivalent ways in which these "naturalness" or "abstractness" conditions have been used.

Lockwood (1975) has shown that both abstract and concrete versions of generative phonology are subject to the same error. Both assume the validity of the derivation axiom and fail to test its consequences. The present study essentially takes off from that point. An alternation in Russian phonology is described on the basis of the syllabification of Russian words. The description is cast in the most common stratificational model because the approach of Hooper 1972 will not work, a point to which I return below. The non-applicability of the alternation condition(s) and Vennemann's weak naturalness condition (WNC) are shown to lead to the strong naturalness condition (SNC). Finally, the SNC is shown to lead to a description which is more abstract and more complex than the presumably abstract phonological description given. The SNC is also shown to miss a significant phonological generalization. Thus the purpose of the SNC is shown to be at variance with its effect. In conclusion, certain metatheoretical implications of these constraints are also studied for their applications to language acquisition and change.

I. The fleeting vowel in Russian

The term *begłyj głasnyj* 'fleeting vowel' refers to a set of Russian morphemes which occur sometimes with, sometimes without a vowel. It is actually a panslavic phenomenon, though the details differ from language to language. The data considered are given in table 1 on the following page. These data focus on the question of defining the position in which there is a vocalic realization as opposed to those in which there is a zero realization. All data concerning the phonemic and phonetic character of the vocalic realization and all accentological data are ignored. They confirm the present description, because in spite of any differences in phonemic realization or phonetic detail, the locus of vocalic realization and the loci of zero realization coincide.

	1	2	3
(1)	stol	'table'	stol
(2)	son	'dream'	son
(3)	sna	'___, g.sg.'	sna
(4)	vo sne	'in a ___'	vos ne
(5)	v son	'into a ___'	vson
(6)	sonnyj	'sleepy'	son nyj
(7)	v sonnyj	'into a sleepy'	vson nyj

Column 1 gives the transcription
Column 2 gives the gloss
Column 3 shows syllabification of the transcribed form

Table 1

The morpheme for 'dream' occurs with a vowel in (2), (5), (6), and (7). It occurs with zero (i.e. without a vowel) in (3) and (4). This problem has been tackled before, but a phonologically general description has never been produced. Generally, the first question asked is whether this phenomenon is to be described by insertion or deletion of a vowel. Both approaches present problems. Vowel insertion requires a prediction of the nature of the vowel inserted, predictions about what might be called attraction of accent, and a description of the environment for insertion. Vowel deletion also requires accentological predictions and description of the environment for deletion. In addition, it requires justification of the nature of the vowel in its underlying form. The accentological problem can be solved in a straight-forward if not trivial fashion once the other problems have been handled, and it will not be dealt with here. But the other problems remain.

The insertion and deletion approaches cannot both be right. D. Gary Miller[2] has observed (personal communication) that all the language acquirer has access to is the vowel-zero alternation present in the paradigm of surface forms. If Miller is correct, insertion and deletion are both wrong. In fact, Miller's observation parallels my own and actually forms the basis of my description.

The data from table 1 are re-worked and presented again in table 2 on the following page. Column 1 gives the underlying forms, with spaces indicating morpheme boundaries. Column 2 gives the glosses. Column 3 presents the surface realizations as a string, and column 4 presents the surface realizations in syllabic groupings. Note that

	1	2	3	4
(8)	stol -	'table'	stolø	(stolø)
(9)	sъn -	'dream'	sonø	(sonø)
(10)	sъn - a	'___, g.sg.'	søn-a	(søn-a)
(11)	vъ sъn - e	'in a ___'	vosøn-e	(vosø)(n-e)
(12)	vъ sъn -	'into a ___'	føsonø	(føsonø)
(13)	sъn ъn ъ j -	'sleepy'	sonønojø	(sonø)(nojø)
(14)	vъ sъn ъn ъ j -	'into a sleepy'	føsonønojø	(føsonø)(nojø)

N. B. The '-' is a desinence boundary marker separating the stem from the ending, if any.
The parentheses in column 4 are not included as overt markers of the syllable boundary--they are just for clarity.

Table 2

syllabification applies to surface forms only. Underlying forms can not be syllabified in the present case, a point to which I return below. Note that there is an underlying *o* in (8) which does not alternate with zero (cf. the genitive singular *stola*). The need for a desinence boundary marker is discussed by Halle (1959:50 and 62-75 passim). Halle uses the symbol '+'. I disagree with much of his treatment, but the fact remains that certain vowel neutralizations occur in the stem but not in the ending. Halle is therefore correct in his claim that the desinence boundary marker is phonologically and morphologically justifiable.

Now consider the alternation between *v* in (11) and *f* in (12) and (14). This is a consequence of the neutralization of distinctive voice in obstruent clusters. It should follow from the zero realizations, and it does. Beyond this comment, the details of the description are ignored (cf. Sullivan 1974).

Finally, consider the use of the symbol ъ. This is the source of the greatest number of possible misinterpretations. Halle 1959 uses the symbol #. But the double cross has since come to be used to represent a word boundary, and some other symbol had to be chosen. Historically, ъ represents a vowel in the Cyrillic alphabet called *jer* which merged with *o* or was lost during the eleventh through thirteenth centuries A. D. It arose from the IE *ŭ (hence Lightner's lax high rounded vowel) via the process of delabialization. I do not suggest that it is or represents either alternative: call it *jer*, *ŭ, an abstract or potential vowel, or a palm tree. It makes no difference whatever. In the stratificational description it is simply a con-

venient label for the line in figure 1 which attaches to the correspondingly-labeled line in figure 5. That line leads down to the phonological alternation between vowel and zero (cf. Miller's comment above). The occurrences of ъ in table 2 are therefore only markers of the position which manifests the vowel-zero alternation. In stratificational description, the labels of lines in linguistic structure have no eschatological significance. They are simply mnemonic devices used to refer to points or relationships within the structure. For this reason, a stratificational description does not claim that Russian still maintains jers (or *ŭ) as (systematic) phonemic entities, even though I use the symbol.

II. Predicting the vowel-zero alternation

In general, a stratification description consists of two interrelated parts: the realizational and the tactic portions. Consider first the realizational portion. This is the part of structure that (ultimately) relates meaning and sound. Roughly speaking, if you are processing the realizational portion of some part of linguistic structure, you are moving from sound to meaning or from meaning to sound.

Now the data of table 2 show two relevant alternations: between *o* and ø (realizations of the morphon ъ) and between - and ø (realizations of the desinence boundary marker). The algebraic rules expressing these alternations are given in (15) and (16). But note that

(15) ъ / o,ø

(16) - / -,ø

there are two neutralizations given in the data: the vocalic realization of ъ neutralizes with the vowel *o*, and the zero realizations of ъ and - neutralize. These neutralizations cannot be explicitly stated by the algebraic statements in any convenient way. They are, however, easily statable graphically, and they are included in the generalized graphic description given in figure 1.

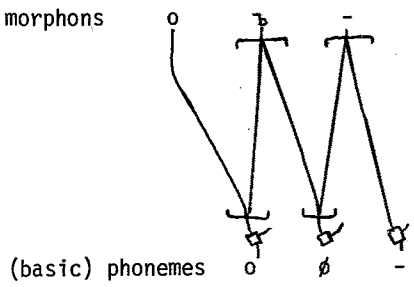


Figure 1: (15) and (16)

The square brackets represent 'or' nodes (rarely if ever used in current transformational-generative models), and the diamonds at the bottom represent the intersection of the realizational portion with the phonotactics (syntax of phonemes). 'Meaning' is somewhere above the top edge of the diagram, 'sound' below the bottom edge. The lines to the northeast from the diamonds indicate tactic connections, i.e. relationships to cer-

tain points or sets of points within the phonotactics. Lines to the southeast from the diamonds indicate relationships to the hypophonemic or phonetic stratum.

The upward 'or' nodes near the bottom of figure 1 show explicitly the neutralization of the vocalic realization of ъ with *o* and of its zero realization with the zero realization of the desinence boundary marker. As mentioned above, (15) and (16) show this, but only implicitly. It is important to reiterate that (15) and (16) are not phonological transformations or rewrite rules, whatever they may look like. They are statements of structure, representing the relational network which is used. But ъ is not rewritten as *o* or as zero during the encoding process. The relationship represented by (15) and by the corresponding lines and nodes in figure 1 is a part of the fixed structure which remains there, whether it is used or not. As Lamb suggests throughout his writing, the logic diagram of figure 1 should be visualized as a network of paths, with the use during the encoding and decoding processes as impulses moving along them.

The remainder of the description concerns the relationship of these possible realizations to the tactic portion of the description. This parallels the statement(s) of environments in transformational-generative rewrite rules. But here again, explicit and implicit are reversed. The explicit relationships of all phonemes to the general statement of the syllable in the phonotactics constitute the total structure. Conversely, the "environment" of a particular realization is implicit in its set of phonotactic relationships.

The main phonotactic generalization is the statement of the syllable, as mentioned above. Haugen made this assertion in his 1956 article, and phonological research on many languages in stratificational, tagmemic, and prosodic models has borne it out. For present purposes, the statement of the syllable given in (17) will suffice.

(17) syl / [onset] peak [coda]

The square brackets indicate optional occurrence. One other piece of information is necessary: vowels are restricted in their occurrence to peak position of the syllable, the only position which is obligatory in the Russian syllable. Note, however, that this statement is specifically about Russian; in other languages, it might not hold true. But in Russian, the consequences are clear. The phoneme *o*, whether it realizes the morphon *o* or the morphon ъ, is related to peak position of the syllable. The non-zero realization of the desinence boundary marker is also related to peak position, preceding the vowel. But its occurrence is optional. The syllable peak is therefore related optionally to an occurrence of the desinence boundary marker which precedes an obligatory vowel. This statement is given algebraically in (18).

(18) peak / [-] (o, ...)

The relationships of (17) and (18) are related to those of fig-

ure 1 in figure 2. Note that the exact descriptions of the relationships connected to onset and coda positions of the syllable are not given. They are by no means trivial; they are simply not relevant to the problem at hand, and are ignored.

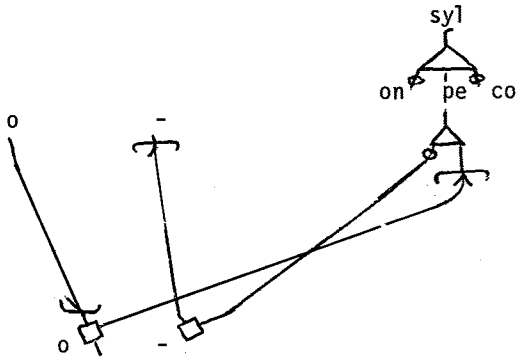


Figure 2: (17) and (18)

Now consider the zero realizations of ʒ and the desinence boundary marker in (8), (9), (11), and (13). All of these realizations occur following the realization of some vowel (whatever its underlying source) in peak position of the same syllable. It might be suggested that a ʒ in contact with a syllable boundary is realized as zero, but a glance at (10), (12), and (14) suffice to show that this is not so. Note that the consonants preceding the zero realizations would be in onset position of the following syllable, if there were a vowel there, as in (9) and (11). The occurrence of the consonants can be described by the addition of an optional syllable-final (SF) element following the syllable and related to onset position of the syllable. The details of the relationship of SF to onset are straight-forward but beyond the scope of this study. Occurring with these consonants in SF position is a zero realization of ʒ or of the desinence boundary marker. No linear order need be specified between the realization of consonants and the occurrence of the zero realization of ʒ or the desinence boundary marker in SF position because no zero realization occupies space in the phonetic chain. The relationship of syllable to SF is given in (19), and the relationship of SF to onset and the realization of ʒ and the desinence boundary marker is given in (20).

(19) macrosyl / syl [SF]

(20) SF / [onset]·ø

The relationships in (19) and (20), when added to figure 2, will suffice to describe (8) and predict the realizations of vowel and desinence boundary marker in (9). To account for (11) and (13), it is necessary only to allow a macrosyllable to be followed (optionally) by another macrosyllable. This additional relationship is described algebraically in (21), where Pword is an abbreviation for phonological

(21) Pword / $(\text{macrosyl})^n$

word. (19), (20), and (21) are added to figure 2 in figure 3.

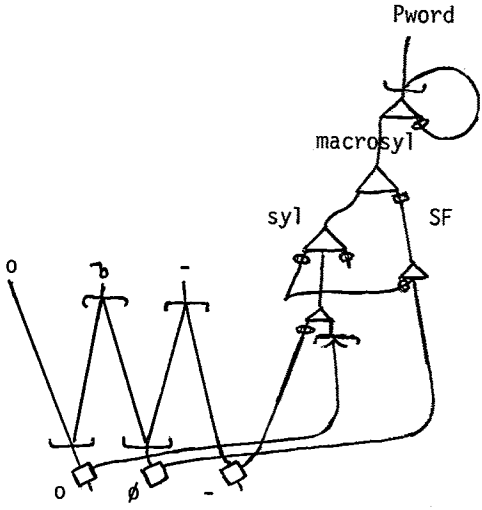


Figure 3: (19), (20), (21)

The node at the top of figure 3 is an ordered 'and' node with its first branch obligatorily leading down to 'macrosyl'. Its second branch is optional, and leads back into 'Pword', i.e. back into itself. This is a recursive element needed, as remarked above, to account for (11) and (13). But it is not actually additional structure necessitated by the alternation. Some such recursive relation is necessary in any event to account for the fact that phonological words and phrases are realized by sequences of syllables in the general case.

Only (10), (12), and (14) remain undescribed. But compare (10) with (9) and (12) with (11). The undescribed portion of (10), (12), and (14) consists of a zero realization of ъ with a consonant. The consonant is the onset of the first syllable of (9) or (11), and is a part of the onset of (10), (12), and (14). This can be described by modifying (21) as in (22), which states that 'Pword' is realized

(22) Pword / $(\text{macrosyl})^n + ((\text{onset}\cdot\emptyset)(\text{macrosyl})^n)$

by an optional occurrence of an onset and a zero realization of ъ followed by an undetermined number of macrosyllables (at least one), if a series of macrosyllables alone does not suffice for a well-formed output. The graphic description is given in figure 4 on the following page. Figure 4 provides a complete description of the data in table 2.

Two questions still need consideration: 1) does figure 4 account for all realizations uniquely, and 2) how does it block the realization of ъ as *o* everywhere. Consider first (14). The underlying form is *vъ sъn ъn ъ j-*, a five-morpheme chain which is realized as *ʃsonnoj*, a two-syllable phonological word. In PC position of figure 4 is the realization of *v* as *ʃ* (not detailed) and the zero realization of ъ. This is followed by the first macrosyllable, which has *s* realized in syllable onset position, ъ realized as *o* in peak position, *n* realized in coda position, and ъ realized as *ø* in SF position. The second macrosyllable has *n* realized in syllable onset position, ъ as *o* in peak position, *j* in coda position, and the desinence boundary marker as zero in SF position. If PC position were ignored, the first ъ would be realized as *o*, the second as zero, and so on. The surface form

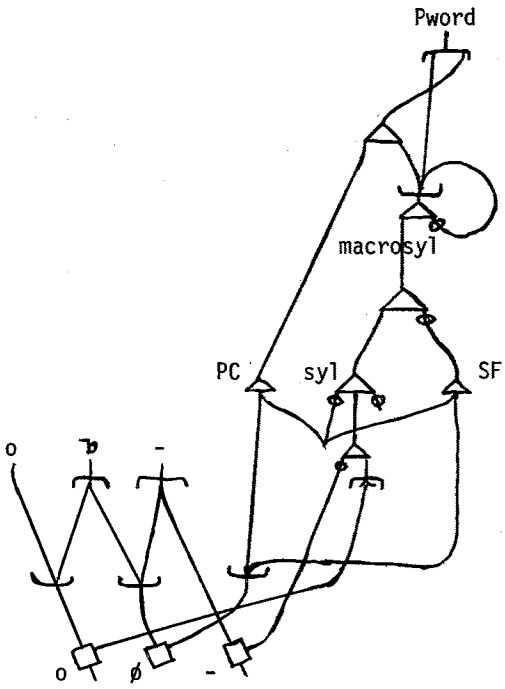


Figure 4: (19), (20), (22)

would be (*voʃø*)(*nonø*)(*j-*). But that would be tactically and phonetically ill-formed, as the final syllable lacks a vowel. Figure 2 (included in figures 3 and 4) states that a non-zero realization of the desinence boundary marker requires a following vowel (cf. also (18)).

The only other possible realization of this chain would have all occurrences of ʒ realized as vowels. The surface shape of (14) would then be *(*vo*)(*ʃo*)(*no*)(*nojø*). This is not phonetically ill-formed and figure 4 does not explicitly rule out the possibility. Implicitly, however, it does. The gestalt nature of the description (and the model and the theory) means that it simply relates ʒ to two possible realizations, *o* and zero. When the phonotactics is processing the Pword, it must do one thing at a time and in the order defined by the structure of the syllable. Faced with a sequence of three sets of realizations of ʒ in a row, it can fit them immediately into PC, peak, and SF positions of the first macrosyllable with the realizations *ø*, *o*, and *ø*, respectively (cf. (12)). Since it can do this, the issue is immediately turned around. Rather than justifying the failure of figure 4 to process these realizations into three separate syllables, it is necessary to justify the need to use three syllables. Processing through one macrosyllable is simpler than processing through three syllables, so from this point of view as well, realizing ʒ as *o* everywhere rather than alternatingly requires justification. Without this justification, just letting the simpler alternative take precedence suffices.

These observations can be formulated into a general principle of network operation: if an Xemic element can potentially have more than one position of realization on a given stratum, the simplest and most natural position of realization is the first one available which allows fulfillment of all obligatory tactic requirements. This is not a constraint. It is, as stated above, an observed general principle about the operation of a relational network description. Circumventing this

operational principle would require extra structure which would itself be subject to some sort of constraint(s). Moreover, this principle explains what happens in those syntactic cases for which the Equi-NP-deletion transformation was introduced. That is to say, the element which has two deep-structure functions but only one surface structure realization occurs only once on each level. On the surface it occurs in the first available position. It is not deleted from the second position because it was never there. The second position remains empty, because by the time the second position is being processed, the doubly-used element is no longer available to fill it. It has already been realized in the first-available position.

In other words, just adopting a relational-network description simplifies the model by reducing the strength and number of assumptions necessary to make it work. But a detailed defense of this claim is beyond the scope of this study.

Parenthetically, this description works when Hooper's approach will not. Hooper 1972 first inserts syllable boundary markers. These then invoke the appropriate Prules and are themselves later deleted. But it is impossible to insert syllable boundaries when the locus of the syllable peak(s) and hence the distribution of the rest of the syllable(s) is exactly what is at issue in (9), (10), and (11).

But one question remains: just where does this description stand on the abstractness scale?

III. Abstractness

Jensen 1974 shows that "abstractness" has been used in several different ways since Kiparsky introduced the concept in its present form in 1968. I am concerned with what Jensen calls the segment paradigm condition (SPC) and the recoverability condition (RC), and with Vennemann's weak and strong naturalness conditions (WNC and SNC). The SPC and WNC are roughly equivalent. Both require that each segment of the underlying form of a morpheme appear as the surface representation of that segment in at least one of the allomorphs, and that if the segment is identical in all allomorphs, it must be the underlying segment as well.

It is immediately clear that *ъ* as an underlying segment violates both the WNC and the SPC: it is neither *o* nor *ø*. Saying that it is a special *o* or a special *ø* solves nothing, since it is phonetically identical to every other *o* or *ø*. But the weakness of both WNC and SPC here is that they both involve an additional assumption, one which is not demonstrably true. Specifically, they assume that "surface" segments (which are, in reality, quite far from sound, the actual surface) have phonetic reality, and that underlying segments share this reality. Even granting such an assumption, however, does not obviate the above description, because *no phonetic character* is claimed for the morphon *ъ*. It is simply the label for the set of po-

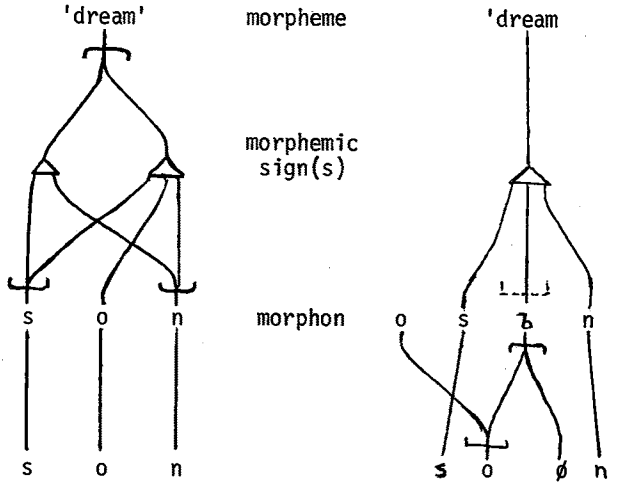
sitions which exhibit a vowel-zero alternation. Moreover, exactly why such an assumption should be granted for other than model-specific reasons has never been made clear. It seems to derive from the set of transformational-generative assumptions about the shape of lexical entries. But that is another question.

The recoverability condition refers to the hypothesis that if a rule which rewrites an underlying segment is lost, then the underlying segment should "resurface". This hypothesis rests on assumptions about the rule that is lost and about the nature of the underlying segment. The rule must be assumed to operate between a particular input and a particular output or not at all. The underlying segment is also assumed to have a unique, non-zero realization. Neither assumption has been justified. Nor have they been stated explicitly, even when they are assumed. In the present description, neither assumption applies. The "rule" of figure 1 relates one point (the morphon *ъ*) to two points (the basic phonemes *o* and *ø*), even though only one is actually realized in any particular instance. The significance of this type of realizational relation is plain. If it were lost, the form that would surface would be either *o* or zero, depending mainly on the acceptability of the syllable which would result. This is a straight-forward phonological prediction, and is exactly what has occurred. The surfacing, so to speak, of zero occurs in *vedьm* 'witch, g.pl.' (cf. Old Russian *vedьmъ*). The vowel also "surfaces". Compare the nominative-genitive singular forms *švec, šveca* 'shoemaker (obs.)' with *kupec, kupca* 'merchant'. In both cases the [*e*]*c* is an agentive suffix with the Old Russian form *ьc-*.

There is another point to consider. If we assume a one point-to-one point type of (rewrite) rule, we would have to write either a deletion or an insertion rule. This has interesting consequences. If it is an insertion rule, its loss predicts generalization of the zero forms. Generalization of the vowel in certain cases is explainable only weakly, as some type of arbitrary relexicalization, for example. Conversely, assuming the loss of a deletion predicts the generalization of the vocalic forms and leaves the generalization of zero forms unexplained. Thus the predictions of the recoverability condition are based on model-specific assumptions about the shape of the rules and even so fail in this instance.

Given the failures of the RC, SPC, and WNC, the natural/concrete school has only the SNC to fall back on. Now the SNC essentially posits one of the surface allomorphs of a morpheme as the underlying form. But the shortcomings of the SPC and the WNC apply here, too. The only way to differentiate between the *o* of *son* and the *o* of *stol*, so that the one can be deleted and the other left alone, is to mark the morpheme. How this differs logically from "marking" the surface *o* of *son* as a "special" *o* escapes me. This latter is considered ad hoc and unacceptable. A parallel argument can be adduced for an insertion rule and a marked zero for *sn*.

We are therefore reduced to listing the allomorphs of *son*. This method of realizing the lexical item 'dream' is given in figure 5A, with the relationships necessary for integration with figure 4 in 5B.



A. under the SNC B. With figure 4

Figure 5

Figure 5 concentrates on the realizational relations between morphemic and phonemic strata and ignores the tactic relations (as given, for example, in figure 4) and all irrelevant realizational relations.

There are several differences between 5A and 5B. 5B is to be preferred for several reasons. First, 5A fails to recognize the fact that the *s* and *n* are not part of the alternation, thus missing a phonological generalization and obviating the possibility of a general prediction of what would happen in the case of rule loss (whatever that might turn out to be here). Note, however, that in this specific instance, the SNC description, unlike the WNC or SPC descriptions, correctly predicts the possibility of a generalization to either *son* or *sn* on an equal basis. 5B, on the other hand, makes the generalization and permits such predictions in the general case, rather than in specific cases only.

Now consider the question of relative simplicity. Both 5A and 5B account for the same data, but 5B is clearly simpler. 5B has a node-line count of 11 to 5A's node-line count of 15. But one case in isolation means little in terms of simplicity. To account for

other cases in the language with 5B, it would be necessary only to add a single 'or' node (the dotted node in 5B). Every additional alternating lexeme would require a new structure roughly equivalent to the portion of 5A between the levels of morpheme and morphon in the allomorph method. The relative simplicity ratio between 5A and 5B can only increase the degree to which 5B is preferable.

Remember, however, that the SNC, unlike the WNC or the SPC, allows a description which accounts for all the data in this specific case, in spite of the cost of the redundancies retained. It falls short relative to 5B in the general case and in simplicity. The WNC and the SPC fail to account for the data.

Ironically, however, the SNC description in 5A also falls short of the description in 5B on the issue of relative abstractness. Remember that the abstractness controversy concerns the distance (however defined) from sound to the "underlying" form. The underlying form in 5A and 5B is represented by the single line leading up toward meaning at the top of the diagram. In 5A it splits alternatively almost immediately at the downward 'or' node. In 5B the downward 'or' node describing the alternation is located at a much lower (or more surface) level--the level of the morphons. This is much closer to sound and is therefore less abstract, more concrete.

The irony of this is compounded if the stratificational description is translated into transformational-generative terms. Under these circumstances, the stratificational description comes out sounding very abstract, if not completely ad hoc. But this is purely an artifact of descriptive terminology. The truth of this claim can be demonstrated by an argument from language acquisition. The language learner is faced not with a special *o* which deletes or a special *ø* which evokes vowel insertion or with an underlying **ū* which does a whole series of things before deleting or neutralizing, but with a vowel-zero alternation. Figure 5B is the most direct possible representation of this datum.

The alternation may be labeled with a ʒ for convenience, as I have done. But it could just as well be labeled with a palm tree or left unlabeled. The central fact is the relationship, not the label. Until this is understood, there is no possibility that the aims and goals of stratificational theory can be understood.

IV. Conclusions

It seems evident to me that some a priori constraint(s) like SPC or WNC (at least) are needed to restrain unbridled appeal to fanciful underlying representations: Lightner suggests (1971: 544), not altogether facetiously, representing *behead* and *decapitate* with one underlying form. As Jensen 1974 points out, however, blind adherance to such a constraint can lead to the rejection of otherwise well-motivated analyses, e.g. Kisseberth's recognition of underlying *u:* in Yawelmani,

even though no such vowel occurs on the surface. Worse, trying to avoid the consequences of such a constraint led Kiparsky (1968:11) to claim that the vowel shift in Chomsky & Halle 1968 results from "low-level, automatic phonetic processes." The issue is apparently irresoluble, as each side adduces arguments which never quite meet those of the other side. The controversy continues.

In stratificational phonology, however, the controversies do not actually arise. In some cases this is true because the relational network description permits a direct assessment of questions of relative completeness, abstractness, and simplicity (cf. figure 5). In other cases, the strict logical construction of the stratificational model avoids the pitfall. Morphons are logical units, lines relating morphemic signs to the phonemic stratum. They are not conglomerations of phonetic features. Nor are they directly related to phonetic features. They do not, therefore, make any claim to phonetic reality, and they can not, therefore, fall afoul of the segment paradigm condition or any other such constraint. All this was discussed above. But I repeat it here preliminary to denying that this is a shortcoming of stratificational theory. It is the SPC which is based on the unproven and untested assumption that underlying segments have phonetic reality and the additional assumption that such phonetic reality is limited in scope to the distinctive feature set in use at some particular time by some particular goup of trend-setting phonologists. Making no claim surely produces a simpler basic axiom set for the theory. In this particular case, the additional claim can be shown to have undesirable consequences. Lamb (1973:147-48) demonstrated that assuming that morphemes are represented directly by feature matrices without an intervening phonemic level necessarily leads to a more complex description.

Finally, stratificational theory does not fall short in the claims it does make. In fact, the basic claim of stratificational theory is more significant and more far-reaching a claim than the SPC, WNC, and SNC can make when all put together. This is the claim that all of linguistic structure can be described on the basis of the (logically) fundamental node relationships and combinations of these relationships *without any additional constraints or assumptions*. The model is highly elaborated and probably difficult for many to read. But the basic axiom set is absolutely minimal. It sounds in this description very much like what Chomsky (1970: 2.2; cf. also his talks to the 1975 LSA Summer Institute at Tampa) calls "a better theory."

This is a strong general claim which a great deal of my work is aimed at testing.

Notes

1. I must thank David Lockwood (Michigan State University) and

Geoffrey Sampson (London School of Economics) for extended discussion on the description given in section II below. Lockwood first formulated the questions on why some of the ill-formed surface shapes do not occur and spotted an error in the diagrams of the earlier draft. Moreover, the major impetus for the theoretical discussions in sections III and IV was given by the paper he presented at the first LACUS Forum (cf. Lockwood 1975). Sampson gave freely of his thinking about possible shortcomings in the stratificational model and cleared up some of my misinterpretations concerning his own thinking. I remain convinced that our reasoning is basically parallel. However, I could still be misinterpreting him. Any remaining errors are, of course, idiosyncratic.

2. I must also thank Gary Miller (University of Florida) for extended discussion on Kiparsky's work. I should apologize for forcing him into the seat of the advocatus diaboli. In spite of his strenuous and generous efforts (the effect of which is implicit in sections III and IV), I remain convinced that where Kiparsky is not wrong, his work is beside the point. That is, it concerns the structure of the model he works in and does not necessarily deal with the structure, acquisition, or change in language as such.

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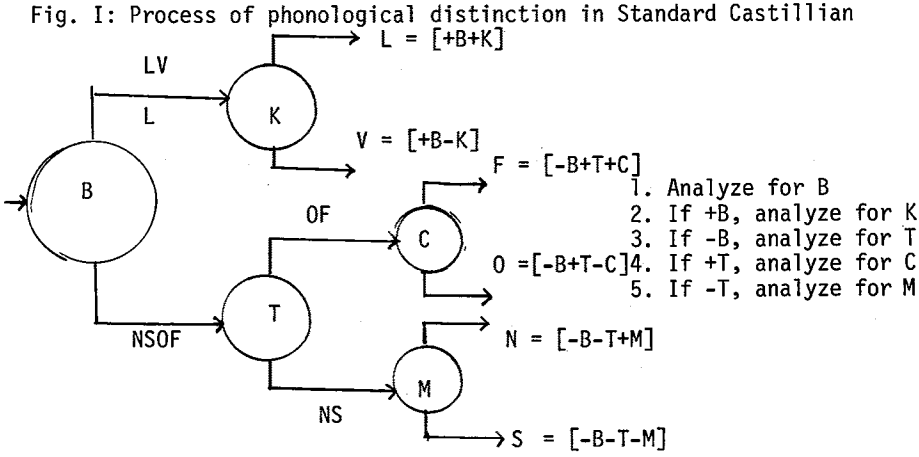
A GENERATIVE MODEL OF SPANISH SYLLABIC STRUCTURES

Francisco Sosa
Lake Forest College

The neuro-sensory process through which phonological distinction is accomplished remains a mystery open to speculation. Physiological studies in this field have only been able to elucidate aspects of the auditive system, but in spite of all the accumulated knowledge on sound perception, we have nothing more than totally unconfirmed theories and hypotheses on the sensorial aspect of phonological distinction. The splitting of phonemes into distinctive features or phones--similar to the splitting of the atom into its constitutive parts--is correspondent to the neuro-sensory decomposition by the ear--by means of a Fourier analysis--of sound waves constituting phonetic realizations. This, of course, would be pertinent for vowels and nasals only, but even then, the consonants' features could be distinguished through the deviations experienced by vocalic components in contact with them.[1]

The process of phonological distinction could be represented by means of a program indicating the order of necessary distinctions to achieve separation of abstract phonic units. The dynamics of this program can be graphically expressed by the passage of phonic current through a system in which each box acts as a filter or separator which analyzes the current reaching it, with respect to whether it contains or lacks the feature of the specific box. Positive current is emitted through the upper part of the box, and negative charge through the lower. The model resembles a chemical analysis with respect to the presence or absence of given radicals.

Fig. I: Process of phonological distinction in Standard Castillian



where B = vocalic; K = consonantal; T = tense; C = continuous; M = nasal.

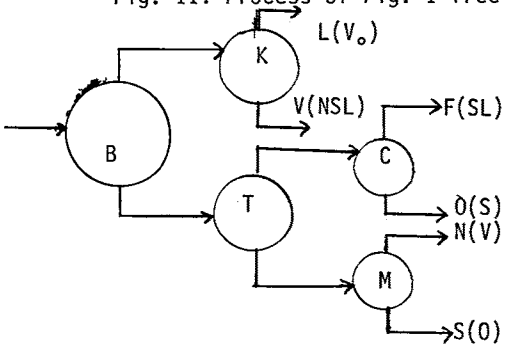
The system operates efficiently. Thus N, S, O and F are not analyzed with respect to K, for this feature--redundant with respect to -B--does not participate in their definition.

The distinctive features which define a given phonological unit constitute the norm which the phonetic realizations of that unit tend to adopt. This norm represents an ideal, neutral solution--free of noise.

The "distinctive" character of these features is quite elastic, for in the absence of redundant features the confusion between phonological units generalizes. Some evidence suggests that upon erasure of context, the information provided by the distinctive features is insufficient to identify phonological units.[2]

The exact definitions resulting from the process simulated here constitute an idealization of reality. The separators do not operate in actuality with absolute efficiency, for they allow the exhaust through the upper part of the box of a fraction of the negative current, and the escape through the lower part of some positive current. In other words, the emitted fluid carries impurities. Incorporating this practical limitation to the process in Fig. I, we obtain a model in which the distinctive features lose their rigorous character.

Fig. II: Process of Fig. I free of idealization



The current escaping the action of the filter explains the existence of variants which contradict the definition based on the norm. It also explains phonological neutralization.[3] There seems to be an apparent contradiction in the definition of [w] and [j] as variants of /u/ and /i/ respectively, for these phonemic units possess the distinctive feature +B, whereas "les glides sont non-vocaliques et non-consonantiques."[4]

In specific contexts the probability of solutions distant from the norm could increase considerably. In conditions in which phonological distinction needs support from the phonetic solution--because of a reduction in redundancy or the presence of noise--the probability p of a given solution is inversely proportional to the phonetic distance separating it from the norm, i.e., the distinctive features defining it.

We could say that a feedback system keeps the solutions--or phonetic realizations--close to the norm, regulating the deviations.

Under external influences--such as contact with another phonological system--or interior developments in the system, the servomechanism may cease to operate, resulting in uncontrolled oscillations until a new normative point of the given feature for the phonemic unit in question is determined, and a new feedback system is stabilized around it. It is thus that true phonetic and phonological changes occur, which are clarified in the diachronic study of the language. It is needless to say that it is not one but many feedback systems which control the operation of phonological units. These controls operate at the level of distinctive features to the nervous system, rather than the individual's consciousness. The latter, if it participates, does it only at the extremes of the communication, being unable to follow in its operation the speeds of nervous impulses.

The innovation of Fig. II rejects the definition S = -T, O = +T, for it hides the neutralization of S and O in syllable-final position The phonetic matrix of S does not require the value -T, nor does O's require the value +T, but rather both of them carry the value ±T. This does not imply that II prevents the distinction between S and O, but rather that it allows this distinction to be maintained for certain values of their respective phonetic matrix, whereas other possible values are undistinguishable, as attested by their neutralization in the proper context. The fact that phonetic matrixes from different phonological levels could produce identical phonic solutions--although with different probabilities--i.e., phonologically undistinguishable solutions, explains the change from one phonological level to another.

A --> A'/_A" means that in the context _A" a solution of A closer to the normative matrix of A' than the one defining A is statistically relevant, i.e., the probability p' of the realization A' of A is such that it cannot be ignored: $p' \gg 0$. This solution A' could even be preponderant in this context: $p' > p_n$, where p_n is the probability of any solution statistically relevant.

Within our model the features defining the different phonological levels V, L, N, S, O and F constitute the electronic configuration of the latter. These features or phones define--as stated--the norm toward which tends the solution of each phonological unit at a given moment in the development of the language. The other solutions which the definition of a given phonological level allows could be considered as isotopes of that phonological plane. In certain contexts the isotopes of a plane may offer the configuration--solution or realization--which is proper to the norm of another plane, resulting in neutralization of two planes which are phonologically opposed. Thus plane Q with normal matrix [aBbK cTdMeC] may present the isotope [a'Bb'Kc'Td'Me'C]--where each of the elements a, a', b, b', etc. represent positive, negative, or zero values of the corresponding phones B, K, etc.--which coincides with the norm of plane Q'.

Phonological model

By using two oppositions--which up to now we have nor considered--as basis, we will present a phonological model of Standard Castillian which indicates the different phonic variants. Taking as coordinates the oppositions acute/grave--x-axis--and dense (compact)/diffuse--y-axis--, we obtain six closed curves gyrating around the imaginary line which projects from the paper starting at the point in which both axes meet. The

six curves move on different planes, corresponding to different values of the third axis: V(vowels), L(liquids), N(nasals), S(soft), O(occlusives) and F(fricatives).

Positioning in this figure the phonemes--discrete units--obtained by means of a classical analysis, we would obtain the points indicated on Fig. III. We could equally catalog the phonemes in a cylindrical table.

Fig. III: Spanish phonemes and phonological planes

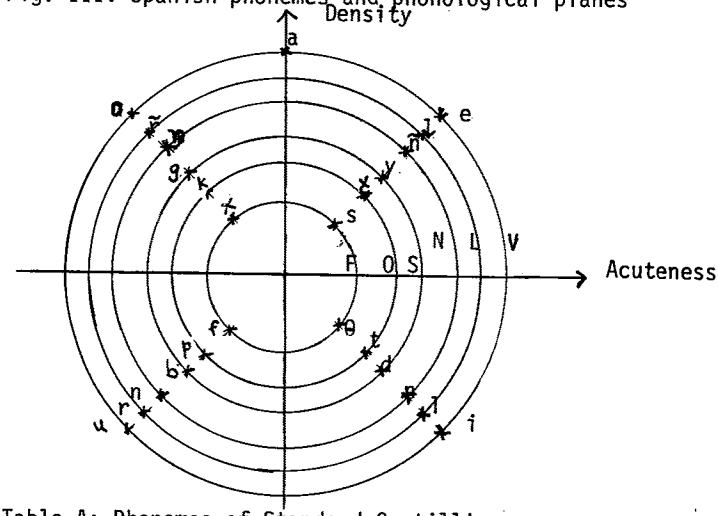


Table A: Phonemes of Standard Castillian

	ve	Acute		Gra	
	Diffuse		Dense		
V	u	i	e	a	o
L	r	l	ʎ		r̃
N	m	n	ñ		[ŋ]
S	b	d	y		g
O	p	t	č		k
F	f	θ	s		x

If instead of tabulating discrete units, we consider each of the Q levels--V, L, N, S, O and F--as a line, we would obtain an infinite number of variants for each of the six abstract phonic units, according to the values expressed by the coordinates along the corresponding curve. By taking notice only of the sign of the coordinates--rather than their exact value--along a given section of the curve, it would be possible to express the classification of Fig. III without recourse to discrete units, based on the phonic value corresponding to each segment of the curve. Calling Q any of the circumferences of the planes, Q_j^i results acute for positive values of i ($i > 0$); grave for $i < 0$; dense for $j > 0$; and diffuse for $j < 0$. Q_j includes positive and negative values of i, as Q^i corresponds to j = (+,-)--positive or negative values of j--. The neutral

values i = 0--neither grave nor acute--and j = 0--neither diffuse nor dense--, corresponding to Q_j^0 and Q_0^j respectively, are found in the limit, in the contact zone between the negative field and its corresponding positive field. For any given Q_j^i, $|i| + |j| = 1$ (the sum of the absolute values of i and j equals the unit).

In Fig. III we not only have the values tabulated in A, but also the intermediate points, corresponding to the different centers of gravity. Thus, the center of gravity--center of gravity of the grave zone, which is also the point of highest gravity--is located at the point i =-1, j = 0; the center of acuteness--center of gravity of the acute zone and point of highest acuteness--is located at (+1,0), the center of density at (0,1) and the center of diffusion at (0,-1). To each of these points corresponds Q_0^-, Q_0^+, Q_+^0 and Q_-^0 respectively.

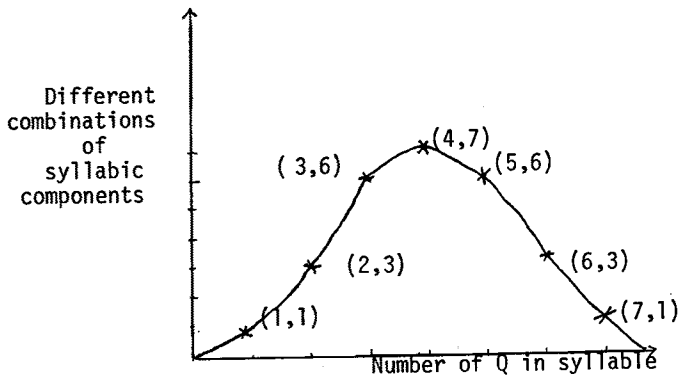
Each of the Q planes thus results in a circle with four sections: each covering a 90° arc, product of the crossing of the 180° arcs Q^-, Q^+, Q_+ and Q_-. This idealization is justified, for the model operates only on distinctive features and relative values. Thus, in the case of Q_j^i --> Q_0^i, density loses its distinctive nature, while the feature i gradually imposes itself, until , in the limit, Q becomes Q_0^{+1}--a Q which is neither dense nor diffuse, distinguishable only through its pronounced acuteness--or Q_0^{-1}--distinguished because of its gravity.

Syllabic Structure

The Spanish syllable must have one or more phonological units, to a maximum of seven. The simplest syllable has one phonological unit, the most complicated syllable has seven.

The curve in Fig. IV shows the kinds of different syllabic combinations plotted against the number of phonemic units in the syllable. As can be seen from the figure, the curve obtained is in the form of a bell, the curve of normal distribution.

Fig. IV



I. Q --> V
V --> (V_+, V_-)

where --> = is resolved as; (a,b) = either a or b.

The simplest syllable has one phonological unit. This unit must be a vowel--either dense or diffuse--, as the presence of V is the condition sine qua non for syllabicity in Spanish. There are as many syllables as full vowels, i.e., charged V_j's. Vowels of neutral (zero) density are not syllabic.

II. Q + Q --> (VV,KV,VK)

In biphonemic syllables there are three possible combinations--VV, VK, or KV--since at least one vowel is required.

a) QQ --> VV

When two vowels come into contact--whether in the interior of a word or with a # (word border) between them--they could be reduced to one, form a diphthong through the conversion of one of them into V_o, or keep a - (syllable border) between them: the vowels being reduced to one syllable in all cases, except the last.

If the two vowels in contact are identical, they are reduced to one vowel of corresponding gravity and density: V_j^i --> $\emptyset/\_V_j^i$. (I.1)

When the vowels are not identical, their reduction to one will be the more complete the greater the similarity between their i and j values. The same vocalic combination could produce different phonic realizations, depending on the degree of relaxation, rhythm, redundancy in semantic distinction, and external conditions under which the declaration occurs. The general equation of compression is reduced in the limit to (I.1)

When the vowels are not compressed, we have:

1) VV --> (V_oV, VV_o)

2) V_oV --> (V_oV_+, V_oV_-) i.e., V --> $(V_+,V_-)/V_o\_$

3) V_oV_- --> $V_o^jV_-^{i'}$

4) V --> $V_+/\_V_o$

In bivocalic syllables one of the vowels will be a full vowel (V_j/ $j \neq 0$) and the other one will be a semiconsonant or semivowel ($j = 0$), according to what is expressed in IIa. IIa3 prevents the combinations [ji] and [wu]; IIa4 prevents the combinations [uj] and [iw].

The reaction of vowels in contact will vary depending on where the stress is according to

5) $V_j + V_{j'}$ --> $((V_j-V_{j'}/\acute{V}_{j'}, V_jV_o)/j > j', (V_j-V_{j'}/\acute{V}_j, V_oV_{j'}))$

where - = syllable border, $\acute{V}$ = tonic vowel, > = greater than.

b) KV

1) K --> (L,N,S,O,F)

2) N --> (N^+,N_-)

3) L --> $(L^+,L_+)/\#\_$

4) V --> (V_+,V_-) from I

IIb2 prevents the combination [bV]; IIb3 prevents [#r]; IIb4 simply follows from I.

c) VK

1) K --> (L,N,S,O,F)

2) F --> F^+

3) $L \rightarrow (L^+, L_-)$

4) $L^+ \rightarrow L^+_+ / \_K^+_+ \quad K \neq F$

5) $N \rightarrow N^i_j / \_K^i_j \quad K \neq F$

6) $S \rightarrow (S_-, S^-)$

7) $O \rightarrow (O_-, O^-)$

8) $S^i_j \leftrightarrow O^i_j$ (reversible reaction)

9) $S^+_- \rightarrow F^+_- / \_\#$

10) $V \rightarrow (V_+, V_-)$ from I

1 through 9 are applicable within the context V_-; IIc8 accounts for the neutralization of S and O at the end of the syllable. As the pronunciation becomes more emphatic the reaction proceeds toward the right, with an O realization; as it becomes more relaxed it proceeds toward the left, giving S as solution.

III. $Q + Q + Q \rightarrow$ (VVV, KVV, VVK, KKV, VKK, KVK)

a) VVV

1) $VVV \rightarrow V_oVV_o$ from IIa

2) $V \rightarrow V_+ / V_o\_V_o$ from IIa

The limitations of IIa require two V_o. They further prevent the combination $V_oV\_V_o$.

b) KVV

1) $KVV \rightarrow (KV_oV, KVV_o)$ from IIa,b (with all the limitations of IIa and IIb.

2) $V^+_o \rightarrow \emptyset / K^+_+\_ \quad K \neq F$

IIIb2 prevents the combinations [čjV], [ňjV], [ľjV] and [yjV]. In cases of such combinations, the palatal semiconsonant is absorbed by the consonant and the combination becomes biphonemic (KV).

c) VVK follow IIa,c

d) KKV

1) $K \rightarrow L\_/-K\_V$

2) $K \rightarrow (((S,O)^-, F^-_-)/-\_L_-, (S,O)^+_-/-\_L^-_-)$

IIId1 requires a diffuse liquid as second element of a consonant cluster beginning a syllable; IIId2 prevents the combinations $(L,N,K^+_+)L$ and (t,d)l.

e) VKK

1) $K \rightarrow F^+_+ / -VK\_-$

2) $K \rightarrow (L_-, N^+_-, (S,O)^+_-, (S,O)^-)/-V\_F^+_+-$

3) $(S,O)^i_j \rightarrow ((S,O)^i_j, \emptyset)$

IIIe1 requires /s/ as second element of a consonant cluster in syllable-final position. IIIe2 shows the possible constituents of the first element of the cluster. IIIe3 indicates the neutralization of O and S in postvocalic position. It further indicates the optional drop

of (S,O) in a syllable-final cluster, reducing the syllabic structure to VK.

f) KVK follow IIb,c

IV. Q + Q + Q + Q --> (KVVV,VVVK,KVVK,KKVV,VVKK,KKVK,KVKK)

a) KVVV follow IIIa,b
b) VVVK IIIa,IIc
c) KVVK IIIb,c
d) KKVV IIIb,d
e) VVKK IIIc,e
f) KKVK IIId,IIc
g) KVKK IIIe,f

V. Q + Q + Q + Q + Q --> (KKVVV,KVVVK,VVVKK,KKVVK,KVVKK,KKVKK)

a) KKVVV follow IVa,d
b) KVVVK IVa,b
c) VVVKK IVb,e
d) KKVVK IVc,d
e) KVVKK IVc,e
f) KKVKK IVf,g

VI. Q + Q + Q + Q + Q + Q --> (KKVVVK,KVVVKK,KKVVKK)

a) KKVVVK follow Va,b
b) KVVVKK IVa,Vc
c) KKVVKK IVd,e

VII. Q + Q + Q + Q + Q + Q + Q --> KKVVVKK[5] follow Va,c

Notes

[1]This is, of course, highly speculative. There are those who deny any value to spectrographic studies, rejecting the simplification presupposed by a Fourier analysis. The main objection would be the inappropriateness of applying linear solutions to non-linear systems. But the antifourierists could go as far as denying that the ear could discriminate vowels. The distinction would take place only at the consonantal level. The vowels--as joining elements between consonants--would be recognized thanks to these: "We have a hunch that in running speech the consonants form the cues for the identification of the words rather than the vowels which are as it were, filled in by the listener on the basis of interpretation" (H. Mol, Fundamentals of Phonetics, I. The Hague, 1963, p. 60).

[2]H. Mol reports a series of experiments on Dutch vowels, Fund.

[3]In the sense of the Prague School.

[4]Roman Jakobson, Essais de linguistique générale, Paris, 1963, p. 128. Cf. R. Jakobson, Morris Halle and Gunnar Fant, Preliminaries to Speech Analysis, Cambridge, Mass., 1952, pp. 19 and 43, and Halle, "On the Bases of Phonology", in Valerie B. Makkai, Phonological Theory, New York, 1972, p. 394.

[5]Example: [trjajns] in nutria inspirada.

SIGN LANGUAGE AND ITS NOTATIONAL SYSTEM

fred c. c. peng
international christian university

A sign language, like any oral language, makes extensive use of body movements. This is evidenced by the fact that speech sounds are produced by the vocal organs and gestures and signs are produced by the hands in relation to other parts of the body. This being the case, a sign language as we know it today may be analyzed along the line of phonetic principles. If phonetics is the branch of oral language study dealing with speech sounds, their production and combination, and their representation by written symbols, we may likewise be able to develop a new science (in the absence of a proper name, I tentatively call it Kinesics *or* Kinetics*) which deals with signs, their production and combination, and their representation by written symbols. In keeping with this analogy, I shall in this paper attempt to follow certain phonetic principles, ultimately arriving at a point whereby gestures and signs can be reduced to written symbols. The symbolization is a notational system on a par with the system of notation used in phonetic transcription. In the course of discussion, I shall rely primarily on Japanese sign language as a source of information. This by no means implies that the system herewith devised is confined to Japanese sign language. Quite to the contrary, it is anticipated that this system of kinesic notation will be enhanced to the status of what I call the International Kinesic Alphabet (*IKA *for short) when other sign languages in the world are tested in the future.*

To begin with, let me divide the human body, from the waist up, into two parts: (X) the hands and (Y) the rest of the upper body. I shall use X and Y as the code names throughout, when there is no need for mentioning the specific body parts involved; further breakdown is necessary, and so a numerical code will be assigned to each subdivision. Reference will be made to a code rather than a descriptive name of the part concerned, whenever I deal with it. The same goes for types.

Given X and Y, it is then possible to classify the signs in Japanese Sign Language (JSL hereafter). Two major types are conceivable: (P) signs made by X in relation to Y and (Q) signs made with X, i.e., one hand in relation to the other. P type consists of five kinds: (1) signs made by X in contact with Y, but involving no positional shift of X on Y; (2) signs made by X in contact with Y, but involving a positional shift of X on Y; (3) signs made by X approaching Y; (4) signs made by X leaving Y; and (5) signs made by X away from Y. Q type consists also of five kinds: (1) signs made by one hand in contact with the other, involving no positional shift of the former on the latter; (2) signs made by one hand in contact with the other, but involving a positional shift of the former on the latter; (3) signs made by one hand approaching the other; (4) signs made by one hand leaving the other; and (5) signs made by one hand away from the other.

It is sometimes difficult, at least at the present stage of the game, to ascertain a sharp demarcation between P5 and Q5. I remain optimistic, how-

ever, that the distinction will be ironed out in the course of my further investigation.

In either P or Q, it is imperative to determine the point of contact each time X comes into contact with Y or one hand comes into contact with the other hand. Since this is only an illustration of my preliminary thinking, coming out of my pilot project, and is neither a formal analysis of JSL nor an explicit presentation of the notational system with which I shall be acutely concerned in the future, I am going to allude in the following to P and P alone which should serve as an explanation of my methodological approach never before stated by any investigator, leaving Q unsaid for the time being. By analogy, Q could be figured out without too much difficulty, however.

In P, note that 5 sets itself apart from the remainder, by virtue of the fact that there is no contact between X and Y. Note further that 3 and 4 are "opposite" to each other in terms of the "direction" (i.e., *approaching* and *leaving*) taken by X in relation to Y. Note furthermore that before X approaches Y — in the case of 3 — or after X has left Y — in the case of 4 — the state of X in relation to Y resembles 5; that is, there is no contact between X and Y. This is an important point to which I shall return later. In the meantime, let it suffice that P may be dichotomized into *Contact Group* and *Non-contact Group*. But I must add at the same time that each group contains full-fledged signs and partial signs.

It is of importance to induce that the *Contact Group* is made up of signs that come from the combinations of X and Y whereas the *Non-contact Group*, of signs that come solely from X. Naturally, then, it is of significance to determine where the contact points are for X and Y.

The contact points for X are: (a) Tip of a finger (or tips of fingers); (b) interior edge (the little finger's side) of the hand; (c) exterior edge (the thumb's side) of the hand; (d) anterior of the hand (the palm); (e) posterior of the hand (the back of the palm); (f) root of the hand (at the wrist). The anatomical position is the point of reference here.

The contact points for Y are: (a) Top of the head; (b) forehead; (c) the back of the head; (d) temple; (e) eye; (f) ear; (g) nose; (h) cheek; (i) mouth; (j) lips; (k) teeth; (l) tongue; (m) chin; (n) neck; (o) shoulder; (p) chest; (q) abdomen; (r) upper arm; (s) elbow; (t) forearm; and (u) wrist.

Since some of the body parts come in pairs, such as the eyes, and since some contact points, for instance, the top of the head, constitute an area, I find it convenient to divide Y into Left, Right, Central, and Both. These will be employed as technical terms in the present context. Only Left/Right distinction is needed for X, however. The terms ipsi-lateral and counter-lateral will also be used for descriptions. These terms were suggested to me by R. Battison when an earlier version of this paper was presented at Gallaudet College (March 18, 1975). By ipsi-lateral and counter-lateral are therefore meant Left/Left or Right/Right and Left/Right or Right/Left combinations, respectively. Thus, it makes no difference whether the signer is right-handed or left-handed. But note that the terms apply only to the description of a symbol engaging one of the combinations in question.

Given these classifications, it is now feasible to describe certain signs with ease, although obviously more detailed classifications will be mandatory to complete the analysis of JSL, such as an account of the *Non-contact Group*, which will follow further below, and that of Q. Suppose, nonetheless, that we take Xa 'tip of a finger (say, right index)' and combine it with Yd 'temple (ipsi-lateral)'. We immediately get a sign that means "to think." Of course, not all contact points of X can combine with all contact points of Y to produce signs in JSL. This constraint should not be misconstrued. For it only means that some of the combinations are not signs in JSL. I shall have more to say in this regard later.

Suppose next that we take the same 'tip of a finger' and combine it with Ym 'chin'. We get, this time, a sign that means "funny, strange." If Xa is moved to Yh 'cheek (ipsi-lateral)', we get a sign that means "false, not true, or lying." If Xa is moved to Yg 'nose', the combination is a sign that means "I."

The same 'tip of a finger' may still be modified to create different signs, such as a *straight* finger versus a *bent* finger. For example, if Xa is a 'straight index', its combination with Yd 'temple' no longer means "to think"; rather, it means "to contemplate, to scrutinize." The sign "to think," therefore, is made by a 'bent index'.

Further differentiation is made through the use of different fingers. For example, if Xa is a 'little finger', its combinations with Ym 'chin' no longer means "funny, strange"; rather, it now means "O.K., agreeable."

From these two instances, it should be clear that the varying shapes and modifying contortions and movements of X play a distinctive role in the configuration of signs, a fact that will be taken into account later. In the meanwhile, two figures are drawn below as a summary.

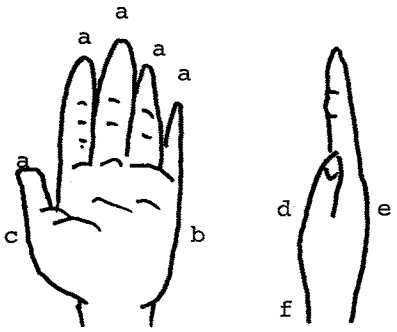


Figure: Contact Points of X

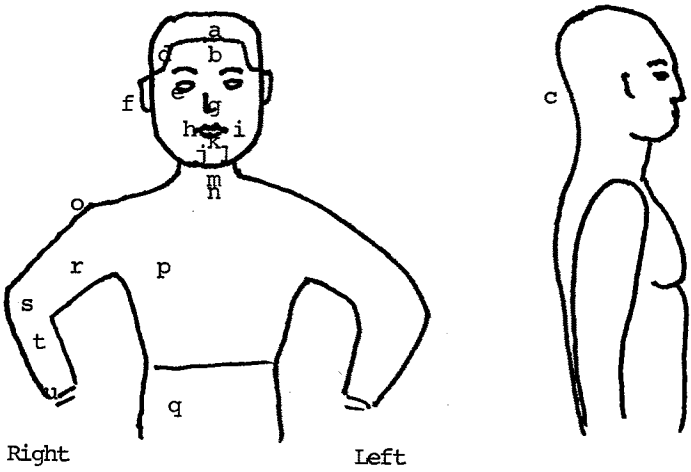


Figure 2: Contact Points of Y

How about the *Non-contact Group*? To account for this group, I need three factors which will determine the relative position wherein the configuration or movement of a sign takes place. It should be emphasized that such a position is part of the sign; the same configuration or movement in a different position constitutes a different sign. The three factors are: (i) distance (from the body); (ii) height (vertical, parallel with the body); and (iii) width (horizontal, parallel with the body).

Distance

Near: closest to the body, flush with the face
Half-way: about one foot away (front or back)
Far: one arm's length (about two feet or so)

Height

High: above the top of the head
Lower-High: from the top of the head to the neck
Mid: the chest area
Low: the abdomen area

Width

Left, Central, and Right (from the signer's point of view)
(*Reverse the order, if left-handed.*)

Given these distinctions, one can fairly accurately determine a position for a configuration or movement. Recall that the position and the configuration or movement together make up a sign in most cases. For instance, a straight index, with the rest of the fingers folded up means "one," if

held in the half-way, mid, and central position (whether the index is held up vertically or laid down horizontally is immaterial) but means "to teach" if moved to the right in the far position with the finger pointing out. In the same position as before, if the configuration is changed, from one finger (index) to two fingers (index and middle finger), the sign now means "two."

From the above-mentioned it follows that configuration in the *Non-contact Group* means either the number of fingers involved or the shape and direction of the finger(s) involved. If a movement is also involved, either in addition to or as part of, the configuration, it can be dealt with accordingly by specifying the beginning and ending point of the movement. These configurational features seem to be shared by the signs in the *Contact Group*. I shall return to the discussion of these features later. In the meantime, let me summarize below with two more figures.

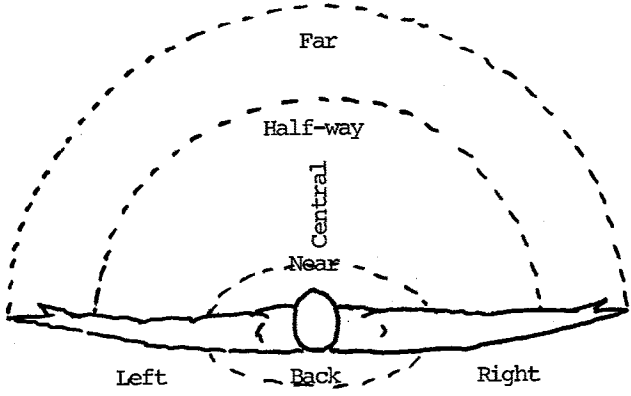


Figure 3: Distance and Width

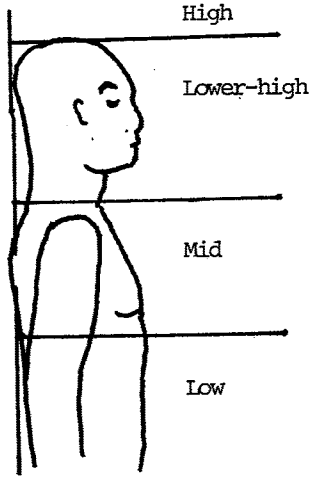


Figure 4: Height

When a linguist is engaged in the analysis of a language that has no orthography (or an orthography which does not represent the sounds involved in any significant way, e.g., Chinese), he is able to account for the facts in the language concerned by way of developing a transcription first, be it phonetic (broad or narrow), phonemic, or morphophonemic. Why? Because there are a number of notational systems available to him, one of which is known as the IPA (International Phonetic Alphabet); he can choose any one of them to write down what he hears for the purpose of analysis. The extent to which the availability of such notational systems facilitates his analysis is rather obvious and, therefore, need not be exaggerated here.

What is important here is that no notational system comparable to the IPA is available to enable an investigator to analyze a sign language. Stokoe's writing system, designed primarily for American Sign Language, is a workable one for mnemonic purposes in recording signs as dictionary entries. To serve the purpose of analysis in the investigation of a sign language, however, demands a more flexible notational system. The significant difference may be illustrated by way of an analogy I see between Roman or Chinese numerals and Arabic numerals. All three are capable of recording any numbers (integers, for the time being). But only Arabic numerals are suitable for analysis (i.e., numerical calculation in mathematics). I wonder how we would go about adding and subtracting, not to mention multiplication and division, by using Roman or Chinese numerals without Arabic numerals. In short, we need a notational system like the IPA in any analysis of a sign language, if that analysis is to be of value in the inquiry of how the deaf acquire the sign language.

Before I apply my basic approach outlined above to the transcription of JSL, it is of necessity that I reiterate the fact that both (oral) language and sign language make good use of body movements and are based on certain body parts for the sending and receiving of signals made up of the movements. This fact, so important in human communication, has unfortunately not been given proper attention thus far. However, I must add that there is a significant difference between (oral) language and sign language. The difference lies in the physical details of the signals: In the case of (oral) language, the signals are noises transformable into sound waves, whereas in the case of sign language, the signals are not, except for such noises as finger snapping, applauding, and lip smacking, etc. Notwithstanding, the sources of the signals are in each case body movements for sure.

The above reiteration seems to suggest that it is not only possible but highly probable to study the body movements that are of importance in sign language more or less in the same vein as we have studied the body movements that are of main concern in (oral) language. For the sake of expedience, I shall call the former *Kinesics* (or *Kinetics*, if there is such a word) and the latter, *Phonetics*.

I see new light in this parallelism which promises a great future. For instance, in phonetics we investigate the speech mechanisms as to how one combines with the others and the extent and manner in which one moves close to or away from the others in order to make sounds. This kind of investigation has been known as articulatory phonetics. If this is the case, we can probably do the same thing by investigating the various body parts as to how one combines with the others and the extent and manner in which one moves towards or away from some others, not to make noises, but to produce movements called signs. I am inclined to call the new kind of investigation "articulatory kinesics."

In phonetics, moreover, there is another branch called experimental phonetics whose object it is to experiment on voices and the muscular controls around the vocal cords, etc., by using certain equipment. When the task is to examine the production of voices by way of sound spectogram, that is, to make invisible sounds visible for a better and more precise quantification, a sub-branch is designated, which is referred to as acoustic phonetics. But when the task is to examine the larynx, observing the fascinating movements of the vocal cords in a state of phonation and the muscular controls related to it by using special equipment such as a fiber scope through a nostril or a pharyngoscope over the pharynx, phonetics becomes akin to physiology. This affinity is reinforced when experimental phoneticians attempt to probe the brain, the nervous system, and the mind, as may be evidenced by the recent move in experimental phonetics. I think an analogy to this aspect of experimental phonetics will be exceedingly fruitful in kinesics. I shall call that analogy "experimental kinesics."

The terms articulatory kinesics and experimental kinesics are pseudonyms for yet to be born disciplines. I am not at all suggesting that there now exists a well-defined methodology to deal with the problems underlying the notational system of sign language. Rather, what I am saying is that if we can "borrow" some of the important concepts from articulatory phonetics and experimental phonetics, I believe a solution is obtainable, simply because in all cases we are concerned with body movements. For instance, [p] is a symbol that stands for voiceless bi-labial stop in articulatory terms. Any phonetician reading that description underlying the symbol [p] will not mistake it for voiced bi-labial stop for which a different symbol [b] stands. If this kind of description is feasible and practical in phonetics, coupled with a set of symbols, why couldn't a similar description be provided (or invented) for articulatory kinesics? I feel this could be done, providing that we keep in mind a set of "kinetic organs" that will be more complex than the set of speech organs with which we are very familiar. If the membership of a fairly comprehensive set of "kinetic organs" can be made certain, at least for JSL, I believe we are well on the way to handle the body movements that characterize JSL.

On a par with experimental phonetics, we can also experiment on muscle coordination, the joints, their structure and function, and so on, and so forth. Prior to this, however, we must assume a fixed position as the point of departure. Knowledge in human anatomy and kinesiology can be borrowed directly or adopted with some modification into experimental kinesics. Once the adaptation is accomplished, we can then conduct experimentation on body movements that are typical of not only JSL but of other sign languages as well.

To explicate the techniques just mentioned, let me now illustrate what I mean by articulatory kinesics to see how much can be accomplished, if my idea is followed. In phonetics, articulatorily speaking, phoneticians find it convenient to separate vowels from consonants. The distinction in the main is based on whether or not the articulators (more movable parts) touch or contact the points of articulation (less movable parts). In other words, if sounds are made by the various contacts of the articulators and the points of articulation, they are consonants, otherwise, they are vowels.

This seems to fall very well in line with my conception of the *Contact Group* and the *Non-contact Group*; I shall call the former "consonants" and

the latter, "vowels." Because kinesic "consonants" are made by the contact of X (which comprises the "articulators") and Y (which comprises the "points of articulation"). These consonants may be further modified by the "manners of articulation" mentioned earlier (cf. straight versus bent index). And, similarly, we can identify the three factors (distance, height, and width) as being equivalent to the three determinants in phonetic vowels, viz., (1) frontness and backness of the tongue, (2) tongue height, and (3) rounding and unroundedness of the lips. To support this analogy, one may draw a chart for kinesic consonants and another chart for kinesic vowels, both of which, however, are omitted here.

Like any oral language, JSL has dialects — both geographically and socially. For instance, the variety of JSL used in Kyoto differs significantly from that in Tokyo. Examples are: "Name" and "Sunday." The "right thumb on the left palm" with the palm facing out in Tokyo means "name" which in Kyoto is signed with a "loop made by the right thumb and index placed on the left chest." "Sunday" is signed with the "right thumb placed on the left palm" with the palm facing up in Kyoto but with "a touch on the lips by the right index and then two hands, palms down, closing together" in Tokyo. I may add, by way of illustration, that the "name" in Tokyo belongs to Q type but is a P-type sign in Kyoto and that "Sunday" on the other hand is a Q-type sign in Kyoto but a composite of two signs in Tokyo; a P-type sign (belonging to the *Contact Group*) and a Q-type sign.

The main difference between older people and younger ones lies in the absence and presence of finger-spelling. This is due mainly to the fact that the older generation was not taught oralism when in school and, therefore, tend to use succinct signs in sequences that differ grammatically from speech utterances, whereas the younger generation, having been taught oralism at school, learn JSL "secretly" from their peers and tend to "spell" particles, such as *de* and *ni*, which JSL lacks as they sign.

Although JSL is the product of the Japanese deaf (i.e., a domestic invention), Japanese finger-spelling is an admixture of foreign (U.S.) imports and domestic modifications. For instance, the *a, i, u, e, o* in Japanese Kana are identical with the five letters in the English alphabet, but the Kana *ho* is a local invention, spelt with a vertical palm slightly bent with its back facing out like a "mast" [ho].

As far as my techniques are concerned, there is no problem in the geographic differences; I will be able to account for the kinesic facts just the same, as if there were no dialectal differences. Finger-spelling, however, will require a separate treatment, since only one hand is employed. But I must add that no one spells a complete "utterance"; after all, only a handful of particles appear in the finger-spelling, which can be taken care of easily by the readily available Kana. More serious, however, is the fact that certain "signs" derive from Chinese characters which are written in the air (called *sora gaki*). Examples are "man," "thousand," and "public." Perhaps, Chinese characters can be used directly as symbols.

If experimental kinesics and articulatory kinesics are upheld in the manner described briefly above, I expect to encounter a great deal of redundancy, a fact that is not so much a pitfall or a limitation of the procedures I propose to use as a desirable necessity in any communication system used

by humans which must be reckoned with, otherwise the description of JSL will be greatly complicated. In phonetics, this is a well-known fact. For instance, when describing the sound [d], the articulator is the tip of the tongue, the point of articulation is the alveolar ridge, and the manner of articulation is stop which is voiced, meaning that the vocal cords are vibrating. The fact that the articulator is said to be the tip of the tongue is because the back of the tongue is lowered and kept away from the palate (or soft palate). The latter feature is an "automatic movement," and therefore does not add anything to the description if mentioned; that is, it is redundant and better off not stated. By the same token, when a deaf person raises a hand to sign, we can regard the hand as a kind of articulator, because the rest of the arm does not add anything to the statement, if mentioned at all. But if we insist that the whole arm be described as well, when our intention is to describe the movement of the articulator, we only complicate our description as the result of our failure to make use or take note of the redundant feature.

In phonetics, it was once believed that one could design one symbol for one sound. The belief was soon shattered to pieces, because it did not take long to realize that the finite set of vocal organs is in fact capable of producing an infinite number of sounds. One solution was the use of diacritic marks which was based on the concept known as componential analysis. For instance, in Mandarin there are four tones which go with the vowels. Suppose that there are nine phonetic vowels in the language. Then, logically, there should be 36 different vocalic nuclei for which 36 symbols must be designed. But if we extract the tones, as it were, from the vowels, and designate them to four diacritic marks, we need to invent only nine more symbols for the vowels, making a total of 13, which is a far cry from 36. Similar "savings" can and must be done in articulatory kinesics. Let me illustrate a few signs from the kinship system (cf. Peng 1974).

Older brother: a touch on the cheek with right index followed by vertical straight middle finger, while the hand moves *up* with the index and the remaining fingers closed.

Older sister: a touch on the cheek with right index followed by vertical straight ring finger, while the hand moves *up* with the index and the remaining fingers closed.

Younger brother: a touch on the cheek with right index followed by vertical straight middle finger, while the hand moves *down* with the index and the remaining fingers closed.

Younger sister: a touch on the cheek with right index followed by vertical straight ring finger, while the hand moves *down* with the index and the remaining fingers closed.

Note that the signs for 'older brother', 'younger brother', 'older sister', and 'younger sister' are made, among other things, with the *middle finger* for the first two and with the *ring finger* for the second two siblings. The distinction within each pair is made by moving the finger in question *up* for the former and *down* for the latter. Thus, if we assign a symbol to the middle finger going up and another to it going down and if we do likewise for the second pair, there should be four symbols corresponding to the four siblings, except that there is one more segment, a touch on the cheek with right

index, which does not concern us here. But if we extract the movement of the finger concerned, as it were, there are two such movements; up-movement and down-movement. These movements, if confined to kinship signs, do not save us any symbols, even when extracted, because two symbols must be designated to them plus two symbols for the two fingers, giving a total of four and we go back to the same number of symbols. Be that as it may, if we go one step further by making use of the symbols assigned to the up-movement and the down-movement for many other signs, such as 'the price going up' and 'the price going down' which are made with two rings (the thumb and index of each hand forming a ring) going up and down, the savings should be remarkable.

Suppose that we assume that I have the knowledge of human anatomy with which to describe what is involved in experimental kinesics when a Japanese deaf-mute makes a sequence of signs. And suppose that I now have a set of symbols with which to operate, knowing all the signs in JSL in terms of articulatory kinesics. Let me then analyze the four kinship signs for the siblings mentioned above.

If we take 'a touch on the cheek with right index' as a segment — whether or not that segment constitutes a sign in itself is beside the point — we can say that it involves both X and Y. X, because it is right index, ipsilateral; and, Y, because it is the cheek. Thus, we can say that it belongs to P, as it is made by X in relation to Y. Moreover, it is Pl, because it is made by X in contact with Y. Hence, it belongs to the *Contact Group*, i.e., it is a "consonant." The articulator of this "consonant" is 'the tip of the index' or Xa and the point of articulation, 'the cheek' or Yh. But, then, without any further differentiation this segment seems identical with the sign meaning "false, not true, or lying" illustrated earlier. But a closer etic examination will soon reveal that the two are articulatorily different, the difference being in the manner of articulation. In the former, the 'touch' should interpreted as 'a flap' whereas in the latter, the 'touch' should be interpreted as 'a push'.

This being the case, we may now designate [/] as the symbol for the segment described as 'a touch on the cheek with right index'. Technically, however, it should be described as a kinesic consonant which is an ipsilateral *tip-cheek flap*.

How about the rest of each kinship sign? Well, the rest of each kinship sign seems to belong to the *Non-contact Group*, be it involving the middle or the ring finger. But, here, the segmentation is a problem regarding the up-movement and the down-movement of each such finger. Earlier, I have mentioned the feasibility of savings in the notation of signs, if we extract those movements from them. Actually, whether or not we extract the movements makes no difference to the fact that the rest of each kinship sign is a "kinesic vowel" by our definition. But suppose that we extract the movements, to see what will happen in symbolization and description. The extraction implies that we treat the vowel as a complex vowel consisting of two segments, a main vowel and a glide. Whatever symbols we employ, the formula expressing each kinship sign in full must then be something like: Consonant-Vowel-Glide. But suppose that we do not extract the movements. This means now that the vowel is a segment in itself. And the formula expressing each kinship sign in full becomes something like: Consonant-Vowel.

In the first formula, the vowel may be described as (1) Half-way, Lower-high, Right Middle Finger (for male siblings) or (2) Half-way, Lower-high, Right Ring Finger (for female siblings). The Glide is up-gliding for the older siblings but down-gliding for the younger siblings. In the second formula, however, the vowel cannot be described in terms of the system I have defined (but can be so described if Figure 3 and Figure 4 are re-designed), although there is no problem in symbolization one way or the other. For this reason, I shall not be concerned with this formula further.

If we take the first segmentation, then let U stand for the first vowel and V for the second. We can designate [^] for the up-gliding, from Lower-high to High, and [ˇ] for the down-gliding, from Lower-high to Mid. But note that each is an off-glide. Adding the consonant designated above, we have the four kinship signs in symbol as follows: [/U^] 'older brother', [/Uˇ] 'younger brother', [/V^] 'older sister', and [/Vˇ] 'younger sister'.

If all signs are transcribed in the way in which the four kinship signs have been transcribed, I can proceed to analyze them scientifically. Note first that morphemically there are five units involved in the kinship signs; namely, [/], [U], [V], [^], and [ˇ]. They have the meanings or carry the semantic components of 'blood relation', 'male', 'female', 'older', and 'younger', respectively. Note next that these morphemes and their semantic units do not necessarily correspond to spoken kinship terms, one to one. For instance, the term for 'older brother' is *ani* (reference only) which is made up of *a-* 'prefix' and *ni* 'older brother' which is a stem. Cf. *ane* 'older sister'. It will be of great interest, once all signs are transcribed, to see how they can be compared morphemically with spoken words

Similar discrepency may be observed, syntactically, between JSL and the Japanese language. For instance, a *woman doctor* in Japanese is *jo i* which corresponds quite well to English in terms of word order but in JSL it becomes *doctor woman*. There are many cases like this, not to mention the omission of particles in JSL. All this will not be understood properly, unless a notational system such as the one I have proposed is provided.

Unfortunately, however, in Japan JSL is discouraged at all costs by the government and downgraded by hearing people, simply because our world is a hearing world and most hearing people have yet to learn from the deaf about their silent world; above all, most people, deaf or otherwise, do not realize that a sign language can be reduced to writing and has grammar just as complex as, if not more so than, that of an oral language. My research in the long run aims at improving the attitude towards JSL in particular and any sign language in general. It also intends to inquire into the intriguing relationship between JSL and Japanese gestures so commonly taken for granted by the Japanese. Perhaps, the IKA may even prove useful to the analysis of the body movements employed in gestures, facial expressions, and bodily contact.

One hundred years ago, scholars had not deemed phonetic transcription possible until the historical meeting of 1886 attended by a small group of language teachers in France who had found phonetic theory and phonetic transcription of value in connection with their work, and who wished to popularize the methods that they had found so useful. If my notational system proves useful at the end of my research, it will in time become the basis of the International Kinesic Alphabet alluded to further above. Today,

we may doubt that the provision of a notational system, such as the IKA, is feasible or even conceivable. But I suspect that in a few years, scholars will be talking about and discussing such a system. When this happens, I believe the IKA will enable them to transcribe kinesically (or kinetically) body movements that can be culturally systematized in the way that the IPA has enabled phoneticians, linguists, and many others to transcribe phonetically sounds that are culturally patterned.

NOTE

This paper was read by title only at the second LACUS conference held in Toronto, Canada. A slightly modified version was presented at the International Congress on Education of the Deaf, held in Tokyo, Japan, August 25 to 29, 1975.

REFERENCE CITED

Peng, Fred C. C.

1974 "Kinship Signs in Japanese Sign Language," in *Sign Language Studies* No. 5.

ORDEREDNESS AND 'AND' NODES*

Earl M. Herrick
Texas A&I University

Stratificational theory, as described by Lamb (1966) and Lockwood (1972), assumes that the structure of individual utterances can be described by diagrams that contain ORDERED AND and UNORDERED AND nodes; and it assumes that the possible utterances of a language can be adequately defined by a diagram that contains these two kinds of AND nodes, ORDERED OR and UNORDERED OR nodes, DIAMOND nodes, ENABLER nodes, and certain other elements, such as reduplication and coordination elements, which may be abbreviations for combinations of these nodes. The author's recent efforts to formalize a description of the graphonomy of English have found some composite relationships which cannot be adequately described by such ORDERED AND and UNORDERED AND nodes. This paper will discuss these troublesome composite relationships, and will present an alternative notation which can describe them more adequately.

Figure 1 gives an example of data which contains some of these troublesome relationships. Because the example is from the graphonomy of its language, the marks which make up the one-word utterance Cindy are themselves the linguistic data to be described. Figure 2 shows the structure of this utterance on the morphemic sign level and on the graphemic level, and also shows the recoding which takes place between those two levels. The DIAMOND nodes, which are the graphemes, have labels with superscript G. Five of these graphemes are letters of the alphabet; the other represents the fact that, for one reason or another, the initial letter of the word is to be capitalized. Although stratificational linguists have understood the orderedness of the ORDERED AND node to be temporal (Lamb 1966:10), this node may be used to represent the spatial ordering of English letters within a text because a literate speaker of English, when asked to "spell" such a text, will name off the letter graphemes in a temporal order which matches their left-to-right spatial order. ORDERED AND nodes are therefore sufficient to show the structure of this utterance on these two levels.

Cindy

Figure 1.

*This paper is a revision and expansion of an earlier paper, entitled "Orderedness and stratificational AND nodes", which was read before the annual meeting of the Michigan Linguistic Society on October 4, 1969. The author wants to thank David G. Lockwood and Charles R. Taber for their comments and assistance. The responsibility for any faults in the present form of the paper must of course belong to the author.

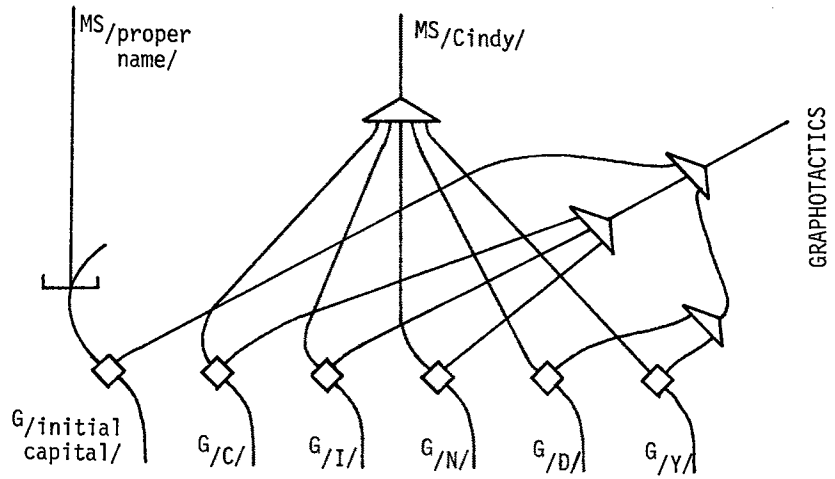


Figure 2.

Each English grapheme which represents a letter of the alphabet is typically realized by two graphemic signs, one of which represents the abstract shape of the mark which embodies the grapheme, the other of which represents the height of that mark with respect to its neighbors.[1] An adequate realization formula for the letter graphemes G/C/, G/N/, G/D/, and G/Y/ is shown in Figure 3. The lines with superscript GS represent graphemic signs; the line which is labeled "/capitalization/"[2] realizes any of several graphemes which can cause a letter to be capitalized. All of the realizations in this formula can be shown by UNORDERED AND nodes and OR nodes.

The realization of G/I/ cannot be described so easily. Whenever it is not capitalized, G/I/ is embodied by two marks, and its realization must therefore include three graphemic signs, two of which represent the shapes of its two marks.[3] Moreover, these marks must be arranged in a certain way in order to embody G/I/; arranged in

[1]See Herrick in press and Herrick 1974.

[2]This symbol, although bracketed by slant lines, does not represent a phoneme. All linguistic symbols in this notation are bracketed by such lines. The symbols which have no superscripts represent points in the network which are convenient for discussion of that network but which have no designations according to the regular system of Xemes, Xemic signs, and Xons described by Lamb (1966:19).

[3]The several marks which can embody G/I/ show great variety, probably greater than for any other grapheme. However, the variety of marks which can embody the lower part of non-capitalized letter I does not affect the argument presented here about the relative locations of its two marks.

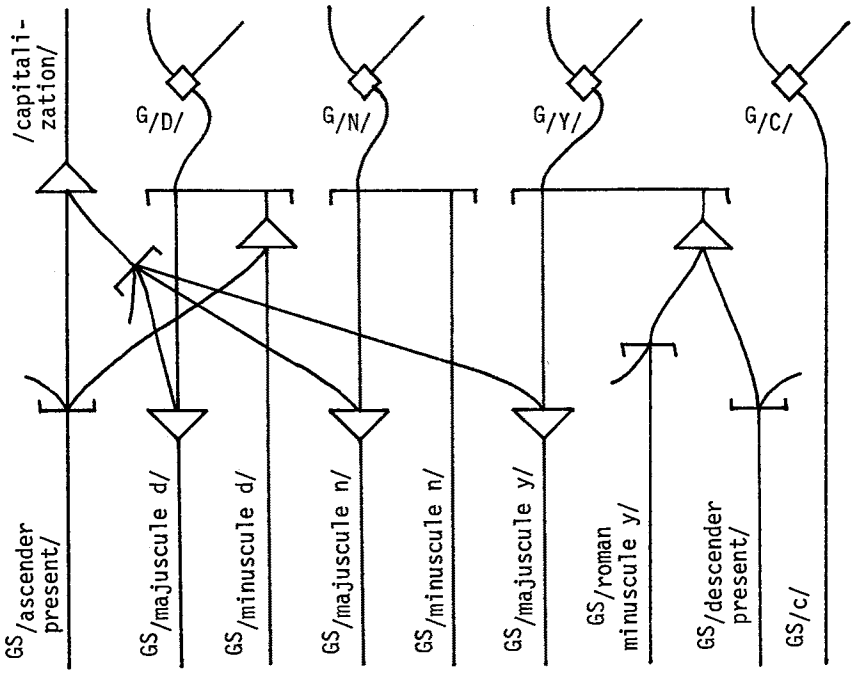
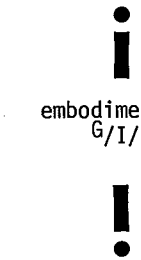


Figure 3.

another way they will embody G/exclamation mark/ (see Figure 4). The diagram in Figure 5, which has a downward UNORDERED AND node at the point marked "/non-capitalized letter I/", is an inadequate realization formula for G/I/ because the part of the diagram below that node could represent either of the pairs of marks shown in Figure 4.

Moreover, the diagram in Figure 5 cannot be made adequate merely by using an ORDERED AND node in place of the UNORDERED AND node. Although the ORDERED AND node may readily be used to represent the left-to-right spatial relationship between the letters of English words, the spatial relationship between the two marks which embody non-capitalized G/I/ is an over-and-under one, and there is no similar way in which the ORDERED AND node can be reinterpreted to represent this relationship. Literate speakers of English do not usually feel that the upper mark embodying G/I/ occurs



embodiment of G/I/

embodiment of G/exclamation mark/

Figure 4.

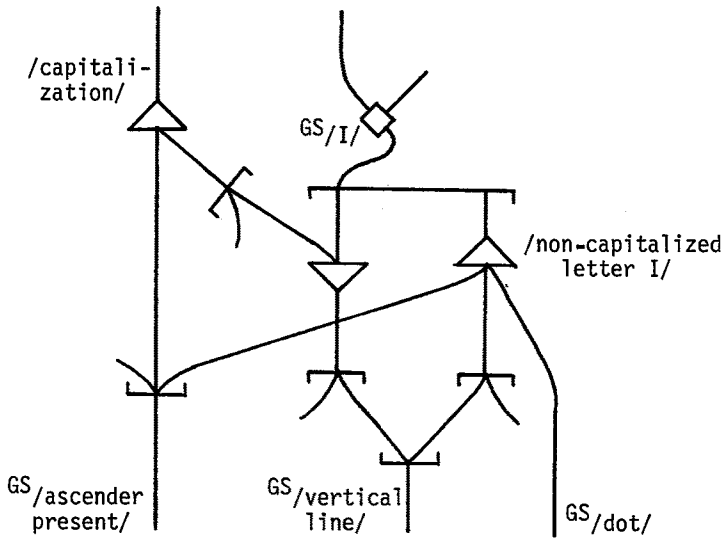


Figure 5.

before or after the lower mark. Therefore, if the ORDERED AND node were used to represent this over-and-under spatial relationship, there would be no principled way to choose between the two diagrams shown in Figures 6(a) and 6(b). Even if some convention were adopted by which

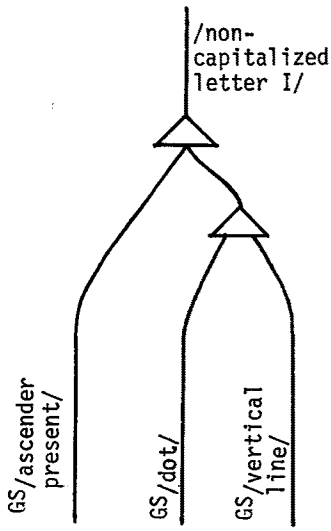


Figure 6(a).

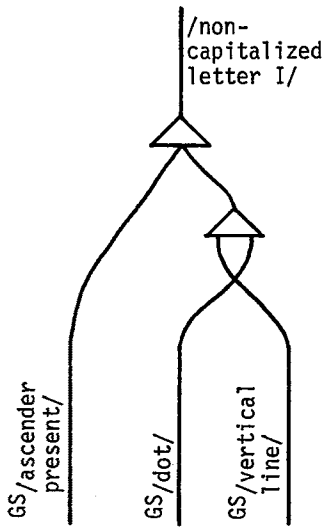


Figure 6(b).

the ORDERED AND node could be used to represent both the left-to-right spatial ordering and the over-and-under spatial ordering, the result would be a troublesome ambiguity. A person reading a stratificational diagram would never be sure, upon encountering an ORDERED AND node, just what it might mean, and might have to do some research in order to deduce its meaning from the rest of the diagram. To avoid this ambiguity, all non-temporal ORDERED AND nodes could be marked with their respective meanings; two possible ways of doing this are shown in Figures 7(a) and 7(b). But whatever notational device may be used, it seems clear that ORDERED AND nodes can represent more than one kind of orderedness.

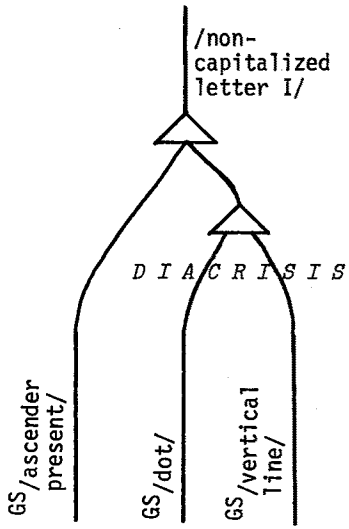


Figure 7(a).

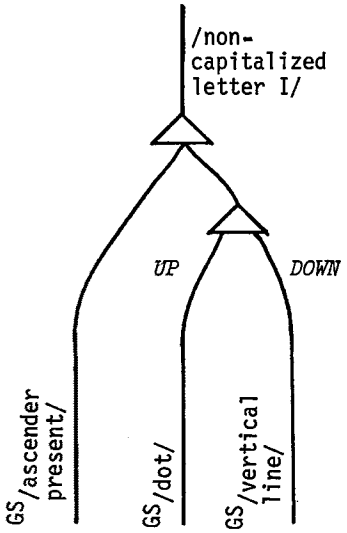


Figure 7(b).

Further down within the graphonomy of English, a similar problem occurs. Graphemic signs that refer to the shapes of marks are realized by graphons, which may be thought of as the contrastive features by which the shapes of these marks are distinguished from one another. Figure 8 shows the shapes which correspond to five such graphemic signs: GS/majuscule n/, GS/roman minuscule y/, GS/majuscule y/, GS/minuscule d/, and GS/minuscule n/. The first three of these graphemic signs are realized according to the formulas shown in Figure 9; the lines with labels with superscript GN represent graphons.

Although the UNORDERED AND node serves adequately for the realization of these three graphemic signs, it will not suffice for the realization of GS/minuscule d/ or GS/minuscule n/. Formulas which use UNORDERED AND nodes to realize these two graphemic signs are shown in Figure 10. Although this diagram does join each graphemic sign to the graphons which realize it, the diagram is inadequate because the reali-

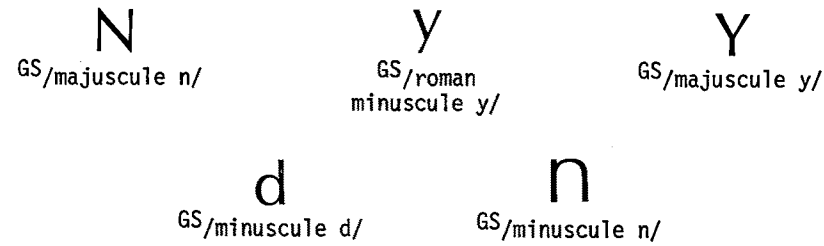


Figure 8.

zation which it shows for GS/minuscule d/ could represent either of the marks shown in Figure 11(a), while the realization which it shows for GS/minuscule n/ could represent either of the marks shown in Figure 11(b). For these two graphemic signs, as for many others, it is not enough to show the graphons which realize them; the relative locations of the parts of the mark described by some of those graphons must be shown as well. As was the case for /non-capitalized letter I/, this relationship cannot be adequately shown by the usual temporally-ordered ORDERED AND node. Furthermore, this relationship among graphons cannot be shown by that node modified as it may have been in order to show the over-and-under spatial ordering. A further kind of ORDERED AND node will be needed.

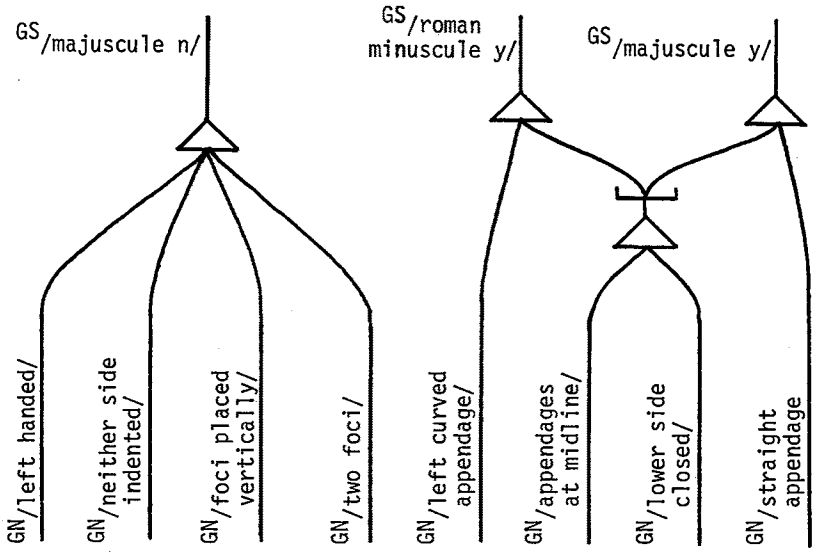


Figure 9.

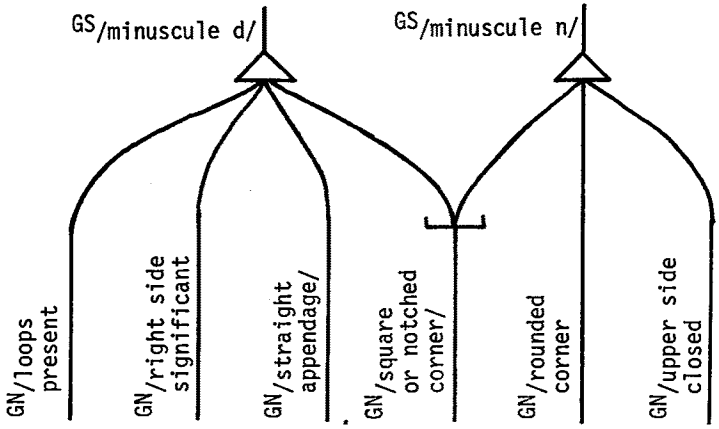


Figure 10.

Because an ordinary mark from the graphonomy of English never has significant shapes at two opposite corners, the significantly shaped parts of such a mark may be partially located by stating the diagonals along which they occur. If the diagonals are named as shown in Figure 12, the realization formulas for GS/minuscule d/ and for GS/minuscule n/ may be adequately written with marked nodes in the manner shown in Figure 13.

We have now seen that the description of English graphonomy requires ORDERED AND nodes which can show at least three different kinds of orderedness, and there is no reason to suppose that further investigation will not find even more kinds of orderedness. Sydney M. Lamb has remarked, informally, that linguists may find it necessary to indicate separately the "ordered" aspect and the "and" aspect of the customary ORDERED AND node. Although when he made this remark he was possibly thinking of orderedness in the usual temporal way, the author would like to take up his suggestion and apply it to the situation which has been described in this paper. How can stratificational networks be drawn so that they will show all the different kinds of composite relationships which can exist within a language?

Figure 11(a).

Figure 11(b).

This may be done by making two changes in the customary stratificational notation. For the first change, the lines from the multiple side of an AND node, whenever they are ordered, will have their orderedness shown by a separate symbol which represents that orderedness as a separate notational unit. Examples of symbols which may be used for

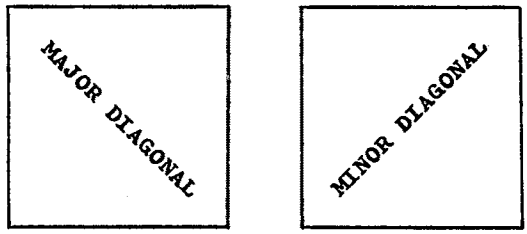


Figure 12.

these units are shown in Figure 14(a); these units may be called "taxemes",[4] and their symbols may therefore be written with a superscript T. Each taxeme symbol consists of an upper part and a lower part: the upper part gives the name of the relationship; the lower part is divided into several sections, each of which gives the name of one pole or alternative of the relationship.[5] From each lower section of a taxeme

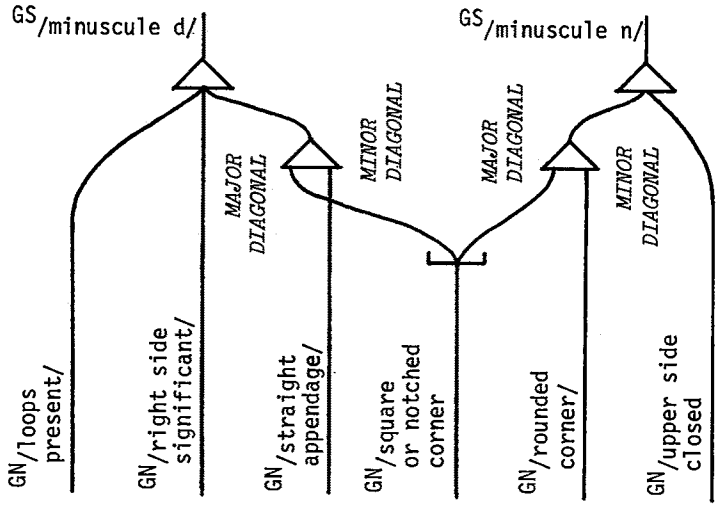


Figure 13.

[4]The word "taxeme" has been used by so many linguists for so many purposes that the author hopes he may be forgiven for using it for yet another purpose. These taxemes are very similar to the "relationals" used by Taber (1966).

[5]It happens that each of the taxemes shown in Figure 14(a) has two lower sections, but there seems to be no reason why a taxeme cannot have three or more.

symbol, a line--which may be called a "valence"[6]--runs to and indicates the thing which is ordered in the way shown by that part of the taxeme symbol.

When written as shown in Figure 14(a), taxeme symbols are cumbersome, and it may be convenient to abbreviate them in the way shown in Figure 14(b). The name of the relationship is then abbreviated between the slant lines, and the names of the poles or alternatives are abbreviated and written around it.

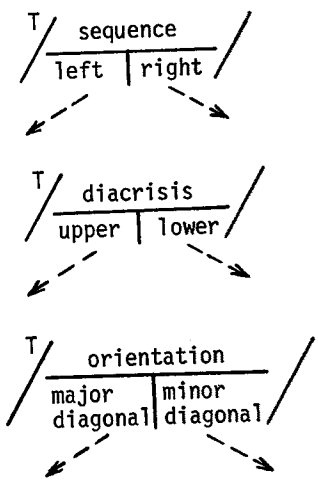


Figure 14(a).

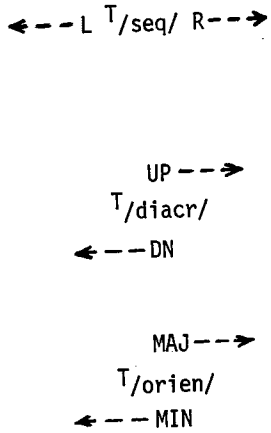


Figure 14(b).

The second notational change becomes possible because every ORDERED AND node will have attached to it a taxeme symbol showing the exact kind of ordering which occurs at that node. The orderedness of the ORDERED AND node is therefore redundant, and stratificational diagrams which include taxeme symbols will need only one kind of AND node which, for convenience, may be drawn in the same way as the former UNORDERED AND node.

These taxeme symbols, which explicitly state the various kinds of linguistic relationships that occur in a language, will make it possible to write adequate substitutes for these inadequate realization formulas. We have seen the difficulty involved in writing a realization formula for $^{G}/I/$. Using a taxeme $^{T}/$diacrisis/ with two valences, the realization of $^{G}/I/$ can be described by the formula shown in Figure 15. In this formula, the node labeled /non-capitalized letter I/ is neither ordered nor unordered; it is merely a downward AND node. The lines which run from this AND node to $^{GS}/$ascender present/, $^{GS}/$vertical

[6]The term "valence" has been used by H.A. Gleason, Jr., for some very similar indicators of relationships in semology. These valences are not the things for which Lamb (1966:59) has used that term.

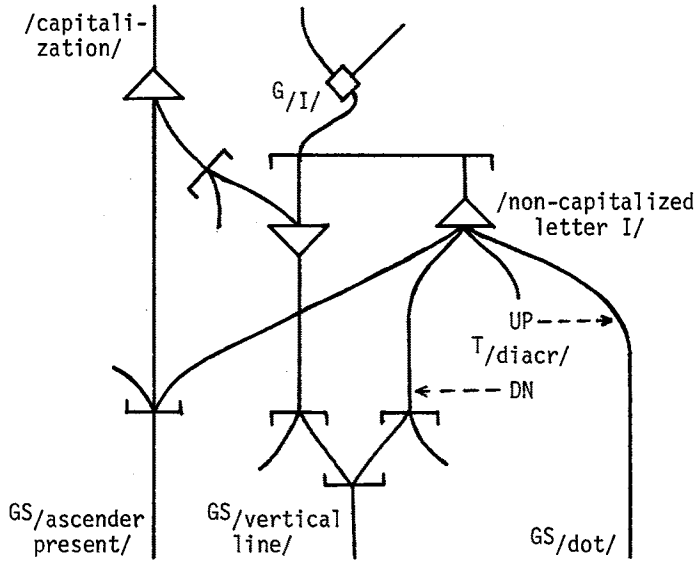


Figure 15.

line/, and GS/dot/ show that those three graphemic signs are included in the realization of /non-capitalized letter I/. In addition, a line runs from that node to a taxeme symbol T/diacrisis/, and two valences run from that symbol--the UP valence running to GS/dot/, the DOWN valence running to GS/vertical line/. This taxeme symbol and its valences which are included in the realization explicitly show the spatial relationship which exists between the two marks that embody /non-capitalized letter I/.

We have seen that the realizations of GS/minuscule d/ and GS/minuscule n/ also involve some difficulties. Figure 16 shows a realization formula which uses taxeme symbols to avoid those ambiguities. In this formula, each of the graphemic signs is realized through a downward AND node, which is neither ordered nor unordered. Some of the lines which run downward from these AND nodes show the graphons which realize each graphemic sign. But in addition, a line runs from each AND node to a taxeme symbol T/orientation/, and the valences which run from each of those taxeme symbols show how the graphons must be arranged to describe its graphemic sign. For both GS/minuscule d/ and GS/minuscule n/, the valences show that the shape of the embodying mark along its major diagonal is described by GN/square or notched corner/. For GS/minuscule d/, they show that the shape along its minor diagonal is described by GN/straight appendage/; for GN/minuscule n/, they show that the shape along that diagonal is described by GN/rounded corner/. Because of these taxeme symbols and their valences, the descriptions on the graphonic level which are given in Figure 16 can represent only the correct shapes of marks among those shown in Figures 11(a) and 11(b).

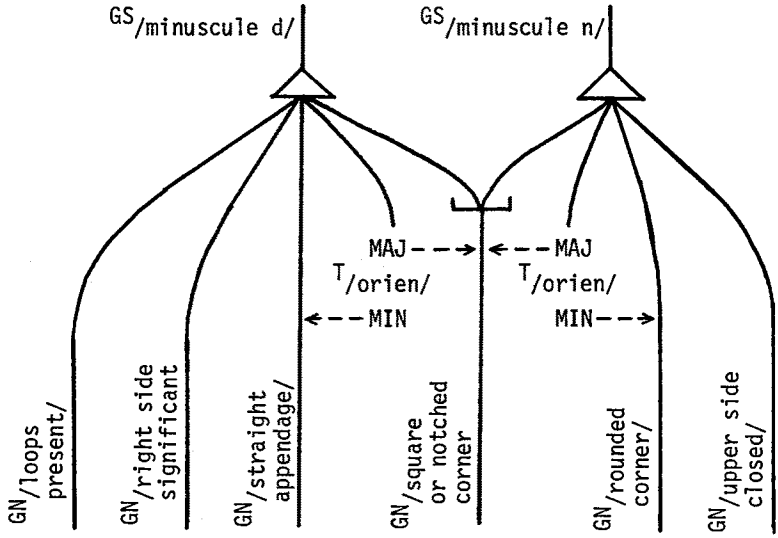


Figure 16.

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ECONOMY AND SYMMETRY IN ENGLISH CONSONANTS

H. A. Gleason, Jr
University of Toronto

The traditional treatment of the English consonant system is in terms of twenty-four phonemes. This analysis gave rise to, and has largely been supplanted by, one in terms of a small set of binary features. It also gave rise to a quite different analysis in terms of *phonons*, developed by Sidney M. Lamb and subsequently elaborated by him and others (Lamb 1964; Lamb 1966, Lockwood 1972:211 et passim). This differs from the more familar treatments in that the features (*phonons*) are unary rather than binary, and in that it grows out of a quite different attitude to universals – that is, to the evidential value in English of observations in other languages. I do not propose to argue this analysis against others, but rather to show how it poses different questions and moulds the argumentation into different shapes.

The traditional treatment can be taken as a starting point. It recognizes twenty-four consonant phonemes. One, /h/, is a manner consonant in the sense of Hockett (1955:96); the remaining twenty-three can be displayed in terms of point of articulation, manner of articulation, and voice. With minimal labelling (figure 1, upper left) there are four points and four manners, but in two places two unpaired consonants or two pairs of consonants fall into the same box. The conventional way of handling this was by a subclassification of manners. In terms of discriminating English consonants the distinction between simple stops and affricated stops is non-functional, but commonly made. A three-way distinction in resonants is usual, though only a two-way division is needed. The resulting system is highly asymmetrical. Seventeen of thirty-two boxes are vacant. If, as was commonly done, points are also cut or subdivided, this is aggravated. Separating bilabials from labiodentals contributes nothing, but adds eight boxes. Separating dentals from alveolars has the same effect, if the spirant manner is already subdivided. One widely used display (figure 1, upper right) has 69% empty boxes.

The guiding consideration here has been phonetic rather than phonologic. The phoneme chart is conformed to the IPA scheme. Phonology has had little influence, once the inventory has been established. Suppose, instead, we shift the weightage toward phonologic considerations, seeking greater symmetry and economy, requiring only that phonetic facts must not be contradicted. And we must raise the question whether the conventional IPA display in three dimensions presents *all* the pertinent phonetic facts.

Joos (1952:222) has pointed out that speakers of American English each use one of two alternative, non-contrastive articulations of /z s/. It has gradually come to attention that the same is true of /r/. Those with retroflex /r/ generally also use tongue-tip /z s/, and those with hunch-backed /r/ generally also use laminal /z s/. This correlation we take as evidence that the three phonemes are related in some way. If we were operating in the now popular way, taking evidence wherever we find it, we might take rhotacism in Old English and a number of other languages as strong further evidence. Restricting our evidence to that of present-day English, where rhotacism is vestigial in *was were*, that is only the weakest support.

There has been a long-continued debate on the status of /ǰ č/ as afficates or clusters. It was scarcely noticed that ortographic *dr* and *tr* pose exactly the same problems, with approximately the same degree of decisiveness. (This was first pointed out to me by William E. Welmers in the fifties.) If /ǰ č/ are treated as affricates, /ɗ ʈ/ should be also.

The asymmetry in the traditional treatment arises from the large number of phonemes or pairs in the apical column, now raised to seven by the recognition of /ɗ ʈ/. But /ɗ ʈ z s r/ show some indications of similarity in articulation, though contrasting in manner (stop, spirant, resonant); they must share a point of articulation. We will not provide a word label for it, but a symbol label **Rz** will serve – it is merely mnemonic of what is included.

Can we give any sort of phonetic specification for **Rz**? We can. This phonon is realized phonetically by the distortion of the upper surface of the tongue from its normal shape. The tip

is retroflexed, or the dorsum humped, or something of the sort. It might be objected that this is not properly a point of articulation. However, something very similar is traditionally so treated in various South Asian languages, within the IPA framework. But the phonologic argument is more pertinent: There is a parallel between /ɖ ʈ z s r/ and /ʃ č ž š y/.

Adding /ɖ ʈ/, dividing the Apical column, and substituting phonon symbols for the minimal labels, produces the display in the lower left of figure 1. It is essentially Lamb's original scheme. Each voiced obstruent or nasal is considered as composed of the two phonons which label the row and column in which it stands. The resonant row is not labelled; we consider these units as composed of one phonon each, e.g. /y/ is simply **Fr**. The symbols may be glossed: **Lb** labial, **Ap** apical, **Fr** frontal, **Do** dorsal, **Cl** closed, **Sp** spirant, **Ns** nasal, or with nominal forms: labiality, closure, etc. **Rz** is read simply /ar zed/ (or dialectal equivalents). (Examples to be cited will require also **Vo** vocalic, **Hi** high, **Lo** low to transcribe vowels, not under discussion in this paper.) **VL**, voiceless, will be discussed below.

	Labials		Apicals		Frontals	Dorsals
Stops	bp		dt			gk
					ʃ č	
Spirants		vf	ð θ			
				zs	ž š	
Nasals	m		n			ŋ
Resonants			l			
				r		
	w				y	

	Bilabials	Labiodentals	Dentals	Alveolars	Prepalatals	Velars
Simple	bp			dt		gk
Affricated					ʃ č	
Flat		vf	ð θ			
Grooved				zs	ž š	
Nasals	m			n		ŋ
Lateral				l		
Median				r		
Semivowels	w				y	

	Lb	Ap	Rz	Fr	Do
Cl	bp	dt	dʈ	ʃ č	gk
Sp	vf	ð θ	zs	ž š	
Ns	m	n			ŋ
	w	l	r	y	

Figure 1. *Upper right:* A traditional display. some labels are incomplete; read "simple stop", etc. *Lower left:* Preliminary display of phonon analysis. *Upper left:* Minimally labelled traditional display. Distinctions used both to the right and below shown —, distinctions used in only one shown ·····.

A phononic transcription consists of a sequence of phonon symbols arranged in groups of one to three with the arrangement within the groups irrelevant. An example follows, contrasted with orthography and a phonemic transcription:

(1)

ph	*o*		*n*	*e*		*m*	*e*
/f	o	w	n	i	y	m	/
Lb	**Lb**	**Lb**	**Ap**	**Fr**	**Fr**	**Lb**	
Sp			**Ns**	**Hi**		**Ns**	
Vl	**Vo**			**Vo**			

The preliminary presentation is a display with twenty boxes, four of them vacant. Each of these holes in the pattern raises some question. The first to examine is that for a frontal nasal. The best candidate is heard in a word like *onion*. The Trager-Smith transcription and a literal trans-symbolization are as follows:

(2)

ə	n	y	ə	n/
Vo	**Ap**	**Fr**	**Vo**	**Ap**
	Ns			**Ns**

This interprets the interlude as a cluster of an apical nasal and a frontal semivowel. However allophonically the nasal is not apical here, but frontal. We might alter the symbol **Ap** to **Fr**:

(3)

Vo	**Fr**	**Fr**	**Vo**	**Ap**
	Ns			**Ns**

But this seems redundant. So we might just eliminate one **Fr**:

(4)

Vo		**Fr**	**Vo**	**Ap**
	Ns			**Ns**

And, if we do this, we might as well combine **Fr** and **Ns** into one segment:

(5)

Vo	**Fr**	**Vo**	**Ap**
	Ns		**Ns**

Here is the frontal nasal. This, (5), is the most economic transcription in terms of number of phonons and number of segments. It also produces a bit more symmetry in the chart by filling one hole.

But is this phonetically justifiable? In our terms of reference it is. I can find no phonetic basis for a segmentation between /n/ and /y/ – though, of course, phonetic basis for any segmentation is admittedly tenuous at best. Articulatorily what I think I do is at least as well described as a single frontal nasal as a cluster.

Why then did the traditional transcriptional analysis not do the same? There are at least two reasons, one historical and one theoretic. The IPA was developed, in the first instance, for English and French, and its transcriptional practice was strongly influenced by the underlying concepts of English-French contrastive phonology. In that tradition much has always been made of the difference between the English sound we are considering and the nearest French equivalent. That long-standing habit is not easily broken. We have been predisposed to a cluster analysis by our heritage.

But beyond that, the economy I find is hardly evident in a phonemic transcription or in a phonemic inventory. Nor is symmetry markedly advanced. In short, within the traditional phonemic framework there is little advantage and some cost. The cost-benefit balance simply works out quite different in the two approaches.

Now consider a second hole, that marked **Rz Ns**. The best candidate is the coda in *barn*. The argument is quite parallel. For those that "pronounce the *r*", the /n/ (of traditional transcription) is allophonically assimilated to the /r/. We may go through steps parallel to (3), (4), and (5), yielding the "missing" segment type and filling one empty box.

(6)

/b	a	r	n/
Lb	**Vo**		**Rz**
Cl	**Lo**		**Ns**

I personally feel that the argument, though of exactly the same form, is not quite as strong for **Rz Ns** as for **Fr Ns**, and I think there is slightly less advantage to it. But it does fill one more hole.

The third hole is quite different. We cannot fill the **Do Sp** box by reinterpreting segments that have traditionally been relegated to allophony, as we did with the "missing" nasals. Velar fricatives are generally considered simply not to occur in English. Here, then, we have to raise a question about the data, not simply the interpretation of agreed upon data.

Consider the familiar name *Bach*. That I pronounce it [bax] is of little consequence; I have been exposed to German all my life. But I hear the same pronunciation with increasing

frequency from monolingual speakers. That is more pertinent. Then there is the common ideophone [yex], and occasionally one with a voiced velar spirant. Admittedly, such evidence is marginal. But we are doing nothing new or unusual if we allow our framework to contribute to a decision on the admissibility of marginal evidence. In the scheme under consideration, there is some real advantage in accepting the set of facts I have sampled as evidence.It fills another hole.

A search of our conventional wisdom and the usual linguistics textbooks turns up nothing which seems immediately apparent as the filler for the last hole. For years I completed presentations such as this by an argument that assumed that there was none. But perhaps traditional phonetics has simply hidden from us what we are seeking, or has labelled it in a way that obscures its place in the system – at least its place in this system. Phonologic interpretation never flows automatically out of phonetic description.

Peter Reich has pointed out to me that there is, indeed, such a hidden candidate (personal communication. See also Reich 1974:355.). The traditional phoneme /l/ is said to have "dark" allophones in certain positions. These are presumably identified as velarized apical laterals. In actual fact for many speakers they are velarized, alright, but have lost their apical laterality entirely. Velarization is left high and dry with nothing under it to velarize! This yields precisely what, from another point of view, would be described as a dorsal resonant. It can be inserted to fill the last hole.

Traditionally this is an allophone of /l/ and some of the descriptions sketch out something of its complementation. I say "sketch out" because I find the statements usually somewhat less finished than, say, with allophones of /t/. I am not sure what a thoroughgoing investigation of the distribution might show; meticulous study of allophonics is a long and arduous process. We have always tended to stop short if results seemed to be corroborating our "native feel" for the phonemics. Why, for example, were linguists so late in discovering the hump-backed /r/? Or some of the phonetic qualities of /dr/? I suspect that there may very well be here some complexity of the sort that impelled me, in 1953, to reject Trager and Smith's (1951) proposal putting together in /h/ voiceless syllable initial and centring-lowering post-vocalic glide. The complementation is very complex and the phonetic similarity tenuous at best. In any case, within the phonon framework that I am discussing, the phonetic similarity between /l/ and /ɹ/ is nil. Each consists of a single phonon, and there is no shared phonon at all.

In support of allophonic status for [ɹ] is the fact that it is always in some sort of interchange with [l]. Careful speech usually shows apical laterality where normal speech has dorsal resonance. Even when it does not, our "native feel" suggests that it should. But this argument is exactly parallel to that which raged in the early fifties over the status of /t/. It too is extremely common in ordinary speech but usually replaced by /ə/ in careful speech, and we generally "feel" that it should be even when it isn't. That line of argument opens up a whole separate topic, which would require another paper.

Whether you accept /ɹ/ as a proven unit or only as a most tentative hypothesis, what we catch a glimpse of is a consonantal system that is completely symmetric, as far as our chart goes. All boxes are filled; every point of articulation combines with every manner, and with zero, unmarked manner.

The only asymmetry is that only obsruents occur in voiced : voiceless pairs, or, putting it another way that **Vl** coöccurs only with **Cl or Sp,** or alone. In any case **Vl** is unique in the system and requires comment.

Lamb's original proposal included the suggestion that the marked obstruents, in English, are the voiceless ones. This is contrary to the more familiar notion. It reflects the different attitude toward the relevance for English of evidence from outside English, usually discussed under the rubric "language universals". In English we are required to have a phonon of the general sort of **Vl** for /h/.It is superficially economical to use it also in distinguishing the voiced : voiceless pairs.

The economy is more than superficial, if it is made to operate in a peculiar relation to the segments, reminiscent of, but not identical with, Firthian prosodies. One occurence of this phonon is realized in every successive obstruent segment. If there is none, it is realized by a segment traditionally /h/. With certain sequences of obstruents **Vl** extends its domain just beyond the release of the obstruent articulation, that is, it is realized not only by voicelessness in the obstruent itself, but also by aspiration immediately following. Granted this way of thinking about it, it is preferable not to combine **Vl** into any segment, but to insert it once before any obstruent sequence. Instead of

(7)	*s*	*t*	*o*	*pp*	*e*	*d*
	/s	t	a	p		t/
	Rz	**Ap**	**Vo**	**Lb**		**Ap**
	Sp	**Cl**	**Lo**	**Cl**		**Cl**
	Vl	**Vl**		**Vl**		**Vl**

we transcribe:

(8)	/h	z	d	a	h	b	d/
	Vl	**Rz**	**Ap**	**Vo**	**Vl**	**Lb**	**Ap**
		Sp	**Cl**	**Lo**		**Cl**	**Cl**

This convention has a small but significant value in that it eliminates one allomorph or one morphophonemic rule for a number of important suffixes. Note, it *eliminates* them, it does not simply account for them.

It also raises a series of important issues. Traditional phonemics described phonemes and their allophones. Occasionally there were diffident proposals for portmanteaux, sequences of phonemes realized by one segment. The most widely familiar was the transcription of syllabic e.g., [n̩] by /ṅ/. Basically, phonemes were segments, their allophones were phonetic varieties of segments, and segmentation was a controlling framework.

Generative phonology inherits the same orientation. Segments are bundles of features. Rules can delete segments, create segments, or copy features from segment to segment. They do not merge segments, nor divide segments, nor blur segments. Feature values change suddenly and decisively, in concert, at segment boundaries. Generative phonologists have come at long last to the place where only underlying features are binary; in the output they are multinary as a convenient representation of gradience. But those segment boundaries remain.

Lamb's treatment of voicelessness is a step away from the tyranny of segments. Reich (1975) is a further, somewhat longer step. I am not sure that it is in every respect the best further step. But at the minimum it does pose some important questions, and opens up the exploration of one possible line of attack. Certainly a great deal more observation, experiment, and thought needs to be directed to the question of segmentation, phonetic and phonologic, and to the relation between the two. It is my conviction that unary features (phonons) are a productive framework within which to explore these and certain other questions. The results might be important beyond the limits of this one mode of statement.

Whether you accept all, or even any, of the phonologic arguments I have sketched is secondary. What I have centrally tried to do in this brief paper is to illustrate how a change of framework changes the questions that are more easily asked, brings new questions to mind, gives different weightage to "facts", changes perspectives. In short, whatever may be its merits as *the* analysis of English consonants – a matter that doesn't interest me greatly – such a treatment by the very fact that it is appreciably different from more familiar ones has a usefulness as presenting new insights that are worth exploring. If it offers nothing more than that, it has justified itself.

The display finally arrived at is as in figure 2. This describes the consonant system in terms of ten phonons which combine in transcription into twenty-one segment types or phonemes. Only seven of the symbols used for phonemes have the same values as in the Trager-Smith system; five do not occur in Trager-Smith.

	Lb	Ap	Rz	Fr	Do
Cl	b	d	ɗ	ǰ	g
Sp	v	ð	z	ž	ǧ
Ns	m	n	ɳ	ɲ	ŋ
	w	l	r	y	ɩ

Vl	h

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TOWARD A SEQUENTIAL ONSET FEATURE PHONOLOGY OF ENGLISH

Peter A. Reich
University of Toronto

Reich, 1975, pointed out the similarity between variation in fingerspelling and variation in the phonology of spoken language. It was found that in one informant two fingerspelled letters differed not in choice and movement of the finger articulators but in the sequential ordering of these articulatory movements. This discovery questions the notion of the phoneme as a simultaneous bundle of features of unit duration. Instead it suggests an alternate phonology which differs from all other phonologies in two important aspects. First, features do not in general appear simultaneously within a phoneme. This is the sequential hypothesis. Second, features are onset features rather than being of unit length. That is, a feature is a signal to the physical articulator to move to a particular position. The articulator moves there and remains indefinitely until another phoneme signals that same articulator to do something else. This is the onset feature hypothesis.

I suggest that a phonology based on these two hypotheses is closer to the articulatory facts than other approaches, is simpler, and leads to very worthwhile insights in all aspects of phonology.

Precedents in previous work

In the not too distant past the study of articulatory features was not considered a part of linguistics. For example, Gleason drew the line between language and extra-linguistic expression at the phoneme, when he said (1961: 265): "Though obviously of concern to linguists for many practical reasons, the allophones stand on the margin of his field of study and are in some respects external to language." Articulatory phonetics was not considered a part of linguistics, but rather a separate discipline upon which linguists depend for certain basic concepts and methods (1961: 239).

This attitude can be traced to Bloomfield (1933), who did not consider the indication of subphonemic detail very worthwhile. Nowadays the attitude is really not very different, except that the basic unit is taken to be the distinctive feature or, within stratificational grammar, the phonemic feature or hypophoneme. Consider the relationship of features to phonemes. Phonemes are generally considered the smallest successive units in language (Hockett, 1951). Features came originally from the names of the rows and columns of the phonemic and phonetic charts, which in turn were determined by considerations of articulatory similarity, distribution patterns, pattern congruity and economy (Hockett, 1942).

Features were a way of cross-classifying phonemes. Thus a /b/ is simultaneously a member of the set of labials, a member of the set of stops, and a member of the set of voiced phonemes. Given enough such cross classifications, of course, they can uniquely define each phoneme. As phonemic theory developed, especially within transformationalism, features became primary and phonemes merely abbreviations for particular sets of features.[1]

[1] Householder (1965) put it thus: "If all men were defined uniquely by the club memberships to which they belonged [then Halle & Chomsky would] say

However, the basic nature of phonology has remained surprisingly constant. As Schane (1973) put it: "That utterances can be represented as a sequence of discrete units is one of the basic theoretical assumptions of phonology. In fact, it is difficult to conceive of any kind of phonological description (or scientific description, for that matter) being made without postulating discrete units."

Chomsky & Halle's phonetic representation consists of a matrix, in which the rows are a set of physically defined features, the columns are phoneme-sized discrete segments, and the values are integers "specifying the degree to which the segment in question is characterized by the corresponding property" (1968: 165). Thus while there is an attempt to move toward a continuum in terms of values of features, there is no consideration of the idea that phonemes might have some internal sequential properties — i.e., that the phoneme may not be the smallest sequential unit.

In recent American linguistics there are two instances in the phonological literature suggestive of sequential features. The first is a proposal by Lamb to handle the famous problem of Russian obstruent clusters. Halle (1959) felt that maintaining biuniqueness required stating a cluster voicing rule twice — once in the morphophonemic rules and once in the phonemic rules. Thus he rejected biuniqueness. Lamb (1966) proposed an alternate solution that preserved biuniqueness without complicating the grammar. His solution was to invent a new phoneme, which had no phonetic reality as an independent segment, but rather was treated as a supersegmental, comparable to tone and pitch. When it occurs, it devoices the entire obstruent cluster.

Postal (1968: 44) pointed out that this solution violated another principle held by Lamb, that of Maximal Reduction, but admitted (1968: 50) that that problem could be overcome by using a voicing feature instead of a devoicing feature as a phoneme. This is the solution preferred by Lockwood (1972: 235-9) and Sullivan (1975). As Carleton Hodge pointed out, this solution yields a non-segmentable phonology (Sullivan, 1975: 517). In this example a feature was made nonsequential because it solved a problem in morphophonemic description, rather than for reasons related to articulatory sequencing.

The other example is given by Krohn (1972). Working within the transformational framework, Krohn pointed out that if the affricates are to be considered single segments, then not all features can be realized simultaneously, because [-continuant] and [+strident] cannot be physically produced at the same time. Furthermore, the order must be that given above or else you have [št] rather than [tš=č]. Krohn notes that Chomsky & Halle's phonology is contradictory in that they propose a principle of complete simultaneity of features when discussing vowel diphthongs, while ignoring it in the case of the affricates. One possible solution would be to question Chomsky & Halle's (1968) analysis of the affricates as single phonemes, but rather than do this Krohn chose an alternate course: "Since the treatment of affricates violates the principle of complete simultaneity [Chomsky & Halle, 1968: 305] of features, and since no evidence has been brought forth to support such a principle, I will assume that it is false (116-117)."

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that only the club memberships are real and the individual men are merely conventional abbreviations."

Krohn then noted that when sequential features are allowed, the vowel diphthongs are much more naturally described. Here we have a clear case of a proposal for a sequential feature phonology, based partly on the actual sequence of articulatory events. However, Krohn did not question the assumption that features end at the phoneme boundary. Furthermore, the only applications that he saw were the obvious cases of diphthongs and affricates.

I wish to argue that what Krohn found to be true of diphthongs and affricates, is true of all phonemes.

The nasals

Let us consider the case of the nasals. There are two allophones of /m/ and /n/. When a nasal follows a sonorant, the opening to the nasal passage *precedes* the oral closure; when it follows an obstruent, the events are simultaneous. The only difference between *kin* as in *kith and kin* and *kiddin'* as in *you gotta be kiddin'* is that in the former there is a nasal opening before the oral closure, whereas in the latter the sequence is reversed. Thus we have a sequential feature minimal pair.

What are the advantages of this analysis? Consider how a phonology of the form Chomsky & Halle (1968) propose would have to handle nasalization. First, there would have to be a rule such as rule 1 nasalizing vowels between nasals.

$$1.\quad [+\text{vocalic}] \longrightarrow \begin{bmatrix} +\text{vocalic} \\ +\text{nasal} \end{bmatrix} \qquad / \ [+\text{nasal}] \ \_\_\_ \ [+\text{nasal}]$$

Second, there would have to be a separate rule to handle partial nasalization. Chomsky & Halle (1968: 165) discuss this in connection with the phonetic representation of the word *inn*. On the feature *nasal* they say that the /i/ has a value "2", indicating that the "degree of the nasalization is partial". Presumably, they would also give the /i/ in *knit* a "2", since in this case, also, "the degree of nasalization is partial". Thus, they would have a rule such as rule 2.

$$2.\quad [+\text{vocalic}] \longrightarrow \begin{bmatrix} +\text{vocalic} \\ 2\text{-nasal} \end{bmatrix} \qquad / \left\{ \begin{matrix} [+\text{nasal}] \ \_\_ \\ \_\_ \ [+\text{nasal}] \end{matrix} \right\}$$

However, these rules as they stand are inadequate and must be modified. For one thing, complete nasalization occurs across arbitrarily long sequences of phonemes between nasals, so long as they are all [+vocalic] -- for example, in *merimac* [mẽr̃ə̃mæ̃k] and *melanin* [mẽl̃ə̃nĩn]. In order to account for this nasalization, rule 1 must be modified, perhaps to rule 1a.

$$1a.\quad [+\text{vocalic}]^n \longrightarrow \begin{bmatrix} +\text{vocalic} \\ +\text{nasal} \end{bmatrix}^n \qquad / \ [+\text{nasal}] \ \_\_\_ \ [+\text{nasal}]$$

I have never been happy with superscript rules such as 1a, because it seems to me that, in fact, they are abbreviations for countably infinite sets of rules. If at all possible, they should be avoided. As I have previously argued (Reich, 1969), they can be avoided in the case of conjoined phrases and clauses, and as I shall show here, they can also be avoided in phonology (for other arguments regarding such rules, see McCawley, 1973). Of this approach we can say three things; first, it's clumsy; second, it fails to describe a significant phonetic fact; and third, it is totally unnecessary.

Its clumsiness is due to having to use at least two rules to describe what is obviously a single phenomenon (see McCawley, 1973: 53). The phonetic

fact it fails to explain is that the partial nasalization in the two cases is phonetically different; in the case of *inn*, the vowel starts off unnasalized and becomes nasalized, whereas the reverse is true in the case of *knit*. It is totally unnecessary, because within a sequential onset phonology, no rules at all are needed.[2] In this system all that is needed is to assume that except for the nasals, the sonorants do not specify anything for the feature nasal; the obstruents all specify [-nasal] prior to any other features, and the nasals all specify [+nasal] before any other feature.

If one assumes that the nasal feature stays on until something shuts it off, all the above examples are handled automatically -- that is, without any rules at all. Notice that this system makes different predictions about when nasalization is turned off, once it is on. Consider the word *innumerable*. Chomsky & Halle would predict that the nasalization would end during the /e/, whereas I would predict it would end during the /ə/.[3] These are predictions that can be determined instrumentally on naive informants. I believe that nasalization does continue in my own speech, but as I have stated before (Reich, 1969) one should not trust one's own intuition in such matters.

Following obstruents, /m/ and /n/ each have allophones in which the nasal opening and the oral closure are simultaneous. This fact was demonstrated to me by my two-year-old son, who did not have this allophone, but instead chose to make use of the prior nasal opening to develop two voiceless nasal fricative allophones of /s/ in such words as *snow* [n̥now] and *small* [m̥mal]. Since the explanation of the development of these allophones lies in the fact of two nasal allophones, differing only in timing of onset of features, this explanation would be overlooked within the framework of a standard phonology.

Consonant clusters

One might argue that like the affricates and glides that Krohn discussed, nasalization is a special case, and should be considered a supersegmental rather than a unit phoneme (for discussion, see Hyman, 1975: 236-238). However, the usefulness of sequential onset features can be seen in other places in the phonology -- for example, in consonant clusters. We know that the articulatory features overlap. For example, in the cluster /bl/ the /l/ is formed by the tongue before the release of the /b/. As far as I know, no phonological theory has ever tried to describe these facts. Within a sequential onset phonology we can do this. One way is to segment the continuous signal into phonemes differently, and make the release of the consonants a part of the vowel that follows. Consider the phrases *sump pump, won't brag, rug dealer,* and *cab manager*. Notice that in these cases there is a sequence of consonants across the word boundary, and in this situation the word final consonant is always unreleased. If this is true in word final position, is it not true in other positions as well? It is also true for medial position -- for example, *stepmother* or *lightning*. What about initial

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[2]As McCawley (1973) points out, a more accurate statement would be that the sequential and onset hypotheses shift the negation rules from rules expressed in the formalism to postulates one assumes in accepting the notational system. The importance of these hypotheses is not that they make the negation rules disappear, but that they represent the negation rules as a part of a more general property of language.

[3]One possibility is that in slow, careful speech, vowels turn off nasality,

position? If we consider the release a part of the following vowel, it accounts not only for coarticulation of consonant clusters, but also for the fact that the initial cluster is influenced by the vowel. Thus this system describes in a natural way the difference of lip shape over the /g/ in *gleem* and *gloom*.

One argument seemingly against this analysis is the difference in aspiration between initial /b/ and /p/. Since this aspiration occurs during lip opening, doesn't this mean that the lip opening is a part of the stop? One answer is to suggest that the voiceless stops are discontinuous constituents. However, there are two parts to aspiration. One is the building up of additional pressure in the mouth; the other is the release. It appears to me that the build-up in the cluster /pl/ precedes the tongue moving into the /l/ configuration, while the release follows it. Thus breath-aspiration appears as part of the /p/, even though it is released later.

The dorsal semivowel

One of the allophones that seems to have been forgotten about (see Francis, 1958: 84) is the dorsal semivowel /L/ as an independent allophone of /l/ in non-initial position, and occasionally as /w/ in word initial position. How often it occurs, and how regularly in various contexts, seems to vary from person to person. Words in which it may occur include: *film, bullshit, help, helm, bulb, calm*. Notice that the so-called "dark-L" within this framework is not a simultaneous constriction in two parts of the mouth; rather it is a "dorsal-L" followed by a "standard-L". Thus *lull* and *pull* are represented as /luLl/ and /puLl/ respectively.

Vowel length

Allophonological considerations suggest that, in English, voiced consonants do not lengthen the preceding vowels; rather unvoiced consonants shorten them. This is because, first, the so-called vowel lengthening not only applies to stops, as the rule is sometimes stated, but also to spirants, nasals and semivowels. The /æ/ is longer in *have* than in *half*; in *ban* it is more like in *bad* than in *bat*; the /a/ in *gar* is more like the vowel in *god* than in *got*. Second, utterance final vowels are also long. For example, the /a/ in *pa* is more like the vowel in *pod* than in *pot*. Finally, when a vowel is followed by a consonant cluster such as /nt/, the vowel is short. Compare *can't* with *canned*. If you postulate that /n/ has a vowel lengthening feature while the /t/ is unmarked for length, then you must explain the short vowel in *can't* by postulating a different allophone of /n/ when followed by /t/. But if you postulate that the /t/ is marked for short while the /n/ is unmarked, then you do not have to postulate a separate allophone or phonemic rule.

The complete system

Let us now take a look at the complete system. It is described in terms of ten dimensions, in which each dimension has one or more values. These are shown in Table 1. They were chosen in such a way that any two

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whereas in rapid casual speech, this feature is one that drops, and thus nasality stays on.

Table 1
Allophonic Dimensions and Their Values

Dimension	Values
Apex of Tongue	Ac (closed); Af (fricative); Al (lateral); Ax (lax); Ap (flap)
Breath	Ba (aspirate)
Dorsum of Tongue	Dc (closed); Df (fricative); Do (open); Ds (semivowel)
Front of Tongue	Fb- (back); Ff- (front); Fn- (central); Fr- (retroflex); F-c (closed); F-f (fricative); F-h (high); F-l (low); F-m (mid); F-s (semivowel)
Glottis	Gb (breathy); Gc (closed); Gv (voiced); Gx (unvoiced, lax)
Labial	Lb- (bilabial); Ld- (labiodental); L-c (closed); L-f (fricative); Lp (partially rounded); Lr (rounded); Lx (open, lax)
Nasal	Nc (closed); No (open)
Pitch	Ph (high); Pl (low); Pm (mid)
Register	Rn (normal); Rw (whispered)
Time	Ts (shorten); Tl (lengthen)

features on that dimension were mutually exclusive — that is, the articulator involved could not be in both positions at once. Of course the position of the front of the tongue and the back of the tongue are interdependent. Thus the /k/ in *keep* and the /k/ in *cool* are in different positions in the back of the mouth because the different vowels which follow position the front of the tongue differently.

Two dimensions (Front of Tongue & Labial) are further subdivided into manner and place subfeatures. Thus there are 4x6=24 possible features for Front of Tongue, and 2x2=4 possible features for the labial consonants.

Three features (Glottis, Pitch & Register) all deal with the larynx. They are separate because they appear to operate separately. One can produce a glottal stop without changing a whisper to a voiced utterance; differences in pitch can occur within a whisper.

Two features (Breath & Timing) are considered instantaneous events which do not continue. Thus a signal to aspirate does not have to be followed by a signal not to; similarly with timing.

Particular allophones do not specify values on all dimensions. Rather they specify values on a few dimensions and take values on the other dimensions depending on context. This has the effect of greatly reducing the number of allophones. For example, the position of the front of the tongue (behind the apex) is quite different in the /l/ of *lea* and *Lou*. However, only one allophone of /l/ is needed, since a front-of-tongue feature is not specified (see Harris, 1974, on anticipatory coarticulation).

Table 2
Sequential Onset Feature Chart of the Major Allophones of English Consonants

		Labial	Labiodental	Apical	Rz	Frontal	Dorsal	Glottal
Stop	Uv	p Ts Nc Gx Lc *tip, spit* p¹ Nc Gx Lc Ba *pit*	p² Ts Nc Gx Ld *leap frog*	t Ts Nc Gx Ac *pot* t¹ Nc Gx Ac Ba *tap*	t² Ts Nc Gx Fmc *star* tr Ts Nc Gx Frc Ba *nitrate*	č Ts Nc Gx Fbc Fbf	k Ts Nc Gx Dc *sack, sky* k¹ Nc Gx Dc Ba *kiss*	ʔ Nc Gc *in* ʔ¹ Gc */mʔm/*
	Vo	b Nc Lc Gv	b¹ Nc Ld Gv *cab fleet*	d Nc Ac Gv d¹ Ap *ladder*	dr Nc Frc Gv	ǰ Nc Fbc Fbf Gv	g Nc Dc Gv	
Spirant	Uv	ϕ Ts Nc Gx Lbf *which /ϕ ʟ i ð̌/ halfpast*	f Ts Nc Gx Ldf	θ Ts Nc Gx Af	s Ts Nc Gx Fmf	š Ts Nc Gx Fbf	x Ts Nc Gx Df *Bach, loch*	h Nc Gb h¹ Gb */mhm/*
	Vo	β Nc Lbf Gv *love birds*	v Nc Ldf Gv	ð̌ Nc Af Gv	z Nc Fmf Gv	ž Nc Fbf Gv		ɦ Nc Gv *ahead*
Nasal		m No Lbc Gv	ɱ No Ldc Gv *comfort*	n No Ac Gv	rn No Frc Gv *barn*	ñ No Fbc Gv *onion*	ŋ No Dc Gv	
Semi-vowel		w Lr Gv wʟ Lr Ds Gv		l Al Gv	r Frs Gv	y Fbs Gv *yes, buy*	ʟ Ds Gv *film, law*	

While the sequential and onset hypotheses are hypotheses about all languages, no claim is made as to the universality of this feature system. These features have been developed to handle the allophonic variation found in English. Such a system for another language, although constrained by the nature of the vocal apparatus, will differ in many details. Even if it turned out appropriate to use some of the same features, they would not necessarily mean the same thing in another language. Thus another language might also have a feature that was usefully described as Apex-closed, but in fact the closure might not be at the same location in the mouth.

Table 2 gives the features for the primary allophones of English consonants.[4] Let us consider the reasons behind some of the feature orderings. The reasons for nasal-close ahead of the other features of stops and spirants have already been given. In the unvoiced stops and spirants the time-shorten feature is placed before the nasal-close feature because the vowel seems to be shortened while it is still nasalized. Breath-aspiration, which is in fact a pressure build-up, must follow nasal and oral closure — otherwise the air escapes rather than builds up pressure. Glottal-voicing in the voiced consonants also appears to lag behind the other features. Initially and after unvoiced phonomes, as in *not very* and *back door* the voicing turns on only after the other articulators are in place.

The affricates are unique among the consonants in terms of onset features in that there is a sequence of two values on the same dimension — namely, manner-close followed by manner-fricative. On the other hand, they are not equivalent to the stop followed by the fricative because the closure is in a different location. Thus this system appears to map a middle course in the controversy concerning whether the affricates are one phoneme or two.

Let us turn to the vowels, shown on Table 3.

Table 3
Sequential Onset Feature Chart of the Simple English Vowels

	Front	Central	Back
Paradigm	Lo [Gv / F] [Do / Ax]		Lp [Gv / F] [Do / Ax]
Hi	i Ffh	ɨ Fmh	u Fbh
Mid	e Ffn	ə Fmn	o Fbn
Lo	æ Ffl	a Fml	ɔ Fbl

I have not yet attempted an extensive investigation into the allophonology of English vowels. The description in this table gives that information necessary to describe their interaction with the consonants. All vowels open the lips, turn on voicing, and open all tongue constrictions. The

[4]For an explanation of the format of this table, see Gleason, 1976.

back vowels open the lips in a partially rounded manner, while the other vowels open them in a neutral manner. Lip-opening precedes glottal-voicing, so that the difference in the shape of the lips over the first phoneme in *keep* versus *cool* can be accounted for without causing voicing of those initial consonants. The back-open feature follows the front of the tongue features in order to account for the different place of articulation of the /k/ in *keep* versus *cool*. In the case of the vowel glides, this system seems to be able to treat them as sequences of phonemes. Thus /iy/ is not different from /i/ followed by /y/. This, of course, says nothing about the underlying morphophoneme, which may be "long-e", or anything else, for that matter.

Given that there is value in this phonology, what does this do to traditional feature phonologies? Are there two separate strata, as Lockwood (1975) suggests? I think not. If you scan Tables 2 and 3, you will readily note that generalizations can be made over sets whose extent coincides with those sets delimited by traditional features. Across two distinct strata, however, the set relations should be different. Rather, what I think has happened is that within phonology we have been confusing set membership with phono-lexical inventory. Within syntax entities such as *noun, verb,* and *adjective* are not part of the lexicon, but rather names for lines in the tactics above disjunction elements -- in other words, names of sets. So I believe it is with *vowel, obstruent, voiced, semivowel,* etc. These are categories that appear in the tactics because in one way or another all the elements so classified behave alike. As opposed to the names for these categories, I suggest that the sequential onset features are the phonological equivalent to the lexical inventory.

In conclusion, I find that thinking about phonology in terms of features that turn on and stay one, and that turn on in a series of steps rather than simultaneously within a phoneme offers one insights into the sound pattern of languages that are not available within other approaches. It also allows us to bring our knowledge about the allophones of our language and about the sequence of articulatory events into our theoretical framework and thus into the mainstream of linguistic work. Such a system, properly developed, will hopefully bridge the gap which has separated phonology from phonetics.

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III

MORPHOLOGY AND SYNTAX

THE IN-GROUP: *ONE* AND ITS COMPOUNDS

Dwight Bolinger
Harvard University

Style, for structural linguists and for transformational linguists too until they invented performance, has been a dumping ground for phenomena that could not be cut to fit the canons of analysis and accordingly did not belong in the System.[1] As our understanding of variation in language grows, it becomes clear that the neighbor's yard is no place to throw our wastes. We must return to the notion of style as that complex of traits within what is defined as a single language whereby the speech or writing of an individual or a group is made distinctive. It may include both the uses of competence—largely a statistical question—and divergences within competence itself. Only with a degree of respect for all kinds of data will it be possible to see the true relationship between the rules that a stylist resorts to and the way he uses them.

In this study of *one* and its compounds, I take a distinction that has been thought to represent only a stylistic difference—suitable to convey a sense of elegance or its opposite, or to avoid monotony—and demonstrate that it answers to a semantic contrast. I offer it as a token of what I believe are many such contrasts—too subtle to be readily noticed and hence dismissed as cases of free variation, something nice for a stylist to play with but unimportant to serious linguistic analysis. I suggest that rather than merely stylistic they are a link between the two fields and need to be approached from either side. For stylists their utility lies in the fact that they define an area in which differences of meaning are so slight that in most practical situations the communicator has a choice and can, without falsifying the cruder aspects of his meaning, play on the nuances to which he knows his audience will respond. For linguists the challenge is to accept that knowledge as proof that they are part of the speaker's competence and to assign them their proper place in syntax or lexicon.

Jespersen (1914: § 17.22) says flatly, "There is no real distinction between the compounds with *one* and those with *body*." He recognizes a few restrictions, such as the fact that only *-one* is normal before partitive *of (every one of our friends, *everybody of our friends)*, the use of *-body* to suggest rank or importance *(a somebody, a nobody,* § 8.441), and the lexicalization of *no one better* in addition to a proverb or two. The restriction where *of* is concerned he attributes to a reminiscence of the numeral value of *one* (§ 17.23). He notes a difference of register: the *one* compounds "are often considered more literary". This is borne out by Carroll (1971), who shows a combined frequency of 761 for the words *anybody, everybody,* and *somebody* and of 1603 for the corresponding compounds with *-one (no one* and *nobody* cannot be included because the spelling of *no one* made it impossible to count).[2] Jespersen shrewdly observes that in Shakespeare the word *nobody* appears chiefly in prose and in the mouths of characters like Falstaff and his friends. There is also a dialectal split that I know too little of to do more than mention; I suspect that the mild register distinction that we have now in American English, where both forms have been pretty well assimilated, may have been based on it.[3]

Differences of colloquiality and lexicalization aside, Jespersen's claim[4] amounts to a denial of any difference in meaning between the compounds with *-one* and those with *-body*. But actually it is fairly easy to shoot down the idea that *-one* and *-body* mean the same, if contexts are selected carefully. The following two situations were posed to a group of thirteen adult speakers of English:

I

"Who's the present for?" I asked.

He gave me an intimate look. "Somebody / "Someone very special, very dear to me," he said. Of course it had to be me, but I concealed my blushes.

II

"Who's the present for?" I asked.

"Oh, somebody," / someone," he said, like meaning it was none of my business. "You don't know him. Her. Them."

"You mean you're not going to tell me."

"That's right."

All thirteen were unanimous in marking *someone* as more appropriate in I, *somebody* in II. The syntactic difference cannot account for the preferences since an exchange like *"Who's it for, somebody special?" – "No, nobody special"* is perfectly normal. If more evidence is needed that *-one* and *-body* are not in free variation, it can be culled from the handbooks. Poutsma (1916: 1302) cites a number of examples to prove the absence of a distinction among which are several that prove the opposite (the remaining prove nothing). The following is cited from Thackeray as an instance of elegant variation:

> She vowed that it was a delightful ball; that there was everybody that every one knew....

If the choice of *-body* and *-one* were semantically indifferent here, one should find it easy to reverse them; but **there was every one that everybody knew* is hardly interpretable.

It is one thing of course to prove that *-one* and *-body* do not mean the same, and another to show what the difference is. The hypothesis that I propose—and on which the test just cited was based—is that *-one* in compounds is still *one*, reflecting not only the numerical value, as Jespersen suspected, but the pronominal value as well.

It is natural for pronouns to embody references to distance from the speaker, to selfness and otherness. This is the prime characteristic of the personal pronouns and the demonstratives. If my hypothesis is correct, it also characterizes the indefinites: *one* and its compounds are marked for closeness to the speaker and for individualization (the pronominal and numerical values of *one* respectively), whereas *-body* is unmarked in these two senses. The *-body* compounds therefore are more like indefinite plurals, less concerned with the speaker-hearer's sense of the identity of the referent. As the unmarked term, *-body* can be used in most of the situations where *-one* can be used, but not vice versa. This can be illustrated by comparing *somebody* to *something* in the following, where 'indefinite' is the overt meaning and *someone* could not be used:

> He must have been cornered by a mugger or a mafioso or somebody, from the way they worked him over.
> He must have been cornered by a grizzly or a cougar or something, from the way he was chewed up.

Where the independent *one* is concerned, most dictionaries recognize some sense of closeness to the speaker. The relevant definitions in the *American College Dictionary* express it clearly: 'a person indefinitely; any one of us all'; 'a person of the speaker's kind; such as the speaker himself'. The use of third-person *one* as a substitute for first person is generally recognized, and even the citations for more inclusive senses reveal that the speaker is apt to be vaguely in focus, as with *One wouldn't like to see that happen,* in the Merriam *Third New International.* There is no independent *body* in American

English to establish a contrast, but *person* will do:[5] *A person wouldn't like to see that happen* destroys the intimacy of the wish. What needs to be shown is that the closeness and individualization of *one* extends to its compounds.

A first test is whether the hypothesis predicts the restrictions noted by Jespersen. First, the partitive. This fits because partitiveness is only another name for individualization. Jespersen's examples are: *some one of the inhabitants, every one of our friends, no one of the company, any one of us* (§ 17.23).[6] Except for *company,* these are all plurals, and we find that plurals typically comprise those sets from which individuals can be separated, i.e., from which a "one" can be extracted. But this may depend on features of the noun. Some singular collective nouns, especially those which like *company* refer to small assemblages, are thought of in terms of individual members:

> Only one of the group (committee, gang, platoon, crew, delegation, cast) was rescued.

This is more difficult with larger assemblages, though the restriction does not depend directly on size; the divisibility–individualization–that permits *one* is interfered with by diffuseness on the one hand *(crowd, population),* to which size contributes, and by functioning unity on the other *(army, retinue),* to which size is unrelated:

> ?Only one of the population (crowd, mob, swarm of people) escaped.
> *One of the army (retinue, police force, harem) was improperly dressed.

The *-one* compounds are used under the same conditions as *one* itself:

> Everyone of the group knew that it had to be like that.
> *What if someone of the crowd fires a gun?
> *No one of the army has a right to follow his own whim.

With non-partitive prepositions the restrictions are lifted and either *-one* or *-body* may be found:

> Only one from the army escaped.
> Everyone (everybody) in the crowd was amazed.
> Somebody among the delegation evidently leaked the news.[7]

Second, Jespersen notes that in the type *No one in the room spoke for some time, -body* would hardly be encountered. But he leaves the reader to infer that it is only this quasi-partitive CONSTRUCTION that is responsible for the restriction, when actually it is at least in part the narrowness of the range. *In the room* designates an intimate group. *Nobody in the world is more patriotic than I am* is a normal sentence. Similarly with a smaller range having vague outlines: *Nobody around here knows anything about it*—this can be contrasted with Jespersen's *every one present, no one concerned in that affair* (§ 17.24), both referring to more tightly knit groups. Similarly, also, sentences containing intensifiers which emphasize inclusiveness: *Nobody (not a soul) in that room spoke for at least an hour; Nobody in the whole class knows more than he does.*[8]

Third, only *-body* is found as a substantive and in corresponding uses of the pronouns that have the sense 'importance':

> You can't join our group if you're just anybody (*anyone)!
> He's a nobody (*no one).
> He really thinks he's somebody (*someone), doesn't he?

Whether this could be predicted from the hypothesis, I don't know; but it at least does not conflict with it: to be emphatically somebody in the world counts, to be nobody there is terrible, whereas against the backdrop of *-one's* more limited universe, the comparison is insignificant. Actually about all that can be said with assurance is that *-body* again behaves like *person* (or *man, woman, boy,* etc.): *He's quite a person.*

The best proof of the hypothesis is how native speakers react to situations that embody a potential contrast. To pursue this I submitted a set of thirty-eight sentences to a group of twelve native speakers of English, who were instructed to choose the form in *-one* or *-body* that seemed more appropriate. Some of the sentences were arranged as minimal or near-minimal pairs in terms of the situations described (specific versus general, personal versus impersonal, close versus distant, etc.). The sentences, with the votes for *-one* or *-body,* appear in Table 1.

TABLE I

Test given 16 February 1973 to twelve native speakers of English, mostly graduate students in linguistics.

Instructions: The compounds with *-one (someone, anyone, no one, everyone)* do not mean quite the same as the compounds with *-body.* The aim of this questionnaire is to try to discover what the difference really is. Try to decide which of the two words given in each sentence seems to fit the meaning better, and put a circle around that word. If you are not sure, make the best guess you can.

1. Help, help, (11)somebody! (1)someone! I'm drowning!
2. What he needs is to have (3)somebody (9)someone as a relative.
3. Ain't (10) nobody (2)no one here but us chickens.
4. Our committee was supposed to have a meeting last night, but (4) nobody (8)no one showed up, not even the chairman.
5. Our committee was supposed to have a meeting last night, but (9)nobody (3)no one showed up, not even the janitor.
6. (A woman is in a crowded department store, looking about her in a distracted manner. I notice her and say:) She looks as if she had lost (4) somebody, (8)someone, maybe her little boy or little girl.
7. They can use just about (10)anybody (2)anyone for that kind of job.
8. (7)Everybody (5)Everyone put on their coats and left.
9. From this whole congregation I would like just one—it can be (5)anybody, (7)anyone, I don't care what he is, mechanic, farmer, plumber, dentist—to step forward and receive God's grace.
10. From this whole congregation I would like just one, you there, or you, or you—it can be (4) anybody, (8)anyone, I don't care who—to step forward and receive God's grace.
11. (Secretary to boss:) (6)Somebody (6)Someone wants to see you. I've no idea who it is.
12. (Secretary, to boss:) (3)Somebody (9)Someone wants to see you. An old friend of yours.

13. (6)Everybody / (6)Everyone loves her in that intimate little circle of hers.
14. (8)Everybody / (4)Everyone loves her—she has fans as far away as New Guinea and Pakistan.
15. (10)"Somebody / (2)"Someone is waiting for you." – "Who is it?"
16. (1)"Somebody / (11)"Someone is waiting for you." – "Who is he?"
17. He must have been cornered by a mugger or a mafioso or (10) somebody, / (2)someone, from the way they worked him over.
18. All together, now, let's (11)EV-rybody / (1)EV-ryone join in singing our national anthem: Oh, say, can you see...?
19. (1)Somebody– / (11)Someone– and you know who—left you this perfumed note.
20. (10)Somebody– / (2)Someone it must have been a pickpocket—made off with the contents of my purse.
21. Let's keep quiet so that (4)everybody / (8)everyone can go to sleep; I'm tired and I expect you could use a good night's rest too.
22. We'll wait till (9)everybody / (3)everyone in the enemy camp is asleep, and then we'll stage our surprise attack.
23. (Her standards are high; she doesn't want just to play around but wants a husband or a serious affair:) She is looking for (3) somebody / (9) someone to love her.
24. (The speaker is not asking merely "Are you going alone?", but is reproaching the other person for his selfishness:) Aren't you taking (2)anybody / (10)anyone with you? [Stress on *with,* not on *any-*.]
25. "Who's coming?" – (11) "EV-rybody. / (1) "EV-ryone. She sent out invitations wholesale."
26. In a class, whenever (6) anybody / (6) anyone is absent, the teacher is supposed to make a note.
27. In our class, whenever (4) anybody / (8) anyone is absent, the teacher makes a note.
28. In our class, whenever (9) anybody– / (3) anyone and I mean ANybody– / ANyone– is absent, the teacher makes a note.
29. I favor (2) somebody / (10)someone nominated by Jones—Roy Smith, to be exact.
30. I favor (7) somebody / (5) someone nominated by Jones, because I respect Jones's judgment.
31. (0) Anybody / (12) Anyone of my friends would be capable of that.
32. (3) Everybody / (9) Everyone put on his coat and left.
33. "We have them surrounded; let (1) nobody / (11) no one escape," he said, with a deadly serious air. "You must not fail."
34. "We have them surrounded; let (9) nobody / (3) no one escape," he said, with a smile. "We'll let them in on the joke later."
35. (Two astronomers have trained a super-powerful telescope on Mars and have been able to bring to view a city with houses, buildings, vehicles, and other paraphernalia of civilization. They assume that it is inhabited by humanoids, but not a single person

is to be seen. One says to the other:) Where IS (10) everybody? (2) everyone? The place looks deserted.

36. (Two astronauts have landed on Mars and come to a city exactly as described in 35. One says to the other:) Where IS (7) everybody? (5) everyone? The place looks deserted.

If you were told, concerning 35 and 36, that *everybody* had to be used in one and *everyone* in the other, how would you assign them?

35. (3) everybody (2) everyone 36. (2) everybody (3) everyone

(Answer this question only if you have used the same word in both 35 and 36.)

37. Of all the people in the world, I don't want (7) anybody (5) anyone but you.

38. Of all the people I know, I don't want (3) anybody (9) anyone but you.

If you were told, concerning 37 and 38, that *anybody* had to be used in one and *anyone* in the other, how would you assign them?

37. (1) anybody (1) anyone 38. (1) anybody (1) anyone

(Answer this question only if you have used the same word in both 37 and 38.)

In virtually all cases the majority favored the choice predicted by the hypothesis. Discussion of the results follows, sentence by sentence.

1. *Someone* would have suggested that the speaker was choosy about his rescuers.

2. Relatives are intimate.

3. Register determines this choice. One of the subjects commented: "In 3 I use *nobody* to keep the substandard style of *ain't.*" This can be tested further with multiple negation: one would not be apt to hear **He don't want no one nohow.*

4, 5. The chairman is part of the in-group, the janitor is not.

6. Mother-and-child relationship is close.

7. *Just about* generalizes.

8, 32. *Everyone* is less like a plural.

9, 10. There are two factors, the closeness of the audience and the greater intimacy of the *you* form of address; in 10, both operate.

11, 12. The person referred to is already close by, but 12 is more intimate.

13, 14. Here it was expected that 13 would yield a majority for *everyone*. Apparently what swayed the vote was the desire to make the reach as great as possible. As predicted, *everybody* was favored more in 14.

15, 16. *He* assumes greater individualization than *it.*

17. This was discussed earlier. *Somebody* has the same degree of generality as *something.*

18, 25. *Everybody* stresses inclusiveness. This overrides the fact that in 18 there is a limited group.

19, 20. In 19 there is a particular person in mind, in 20, not.

21, 22. Speaker and hearer are included in the first, which is also probably a more limited group. In 22, speaker and hearer are excluded.

23. As in 1, *someone* suggests selectivity.

24. Selfishness means lack of concern for another person as an individual.

26, 27, 28. In 26, the generalization about *a* class, indefinite, is counterbalanced by the closeness of the group and the ease with which the speaker can identify it with a particular instance. The balance is tipped toward intimacy in 27 with *our,* and toward generality in 28 with emphasis on inclusiveness, "no exceptions".

29, 30. In 29 the speaker has a particular person in mind. He might or might not personalize the reference in 30.

31. This is the partitive, the only instance in which *-body* appears to be "ungrammatical".

33, 34. I believe that there are two factors in this pair. The first is the choice of the more elevated form in the more serious situation. The second is the individualization in 33.

35, 36. Geographical distance is decisive. It is hard to think of an in-group unless one is on the spot.

37, 38. In 37, the intimacy of *you* is counterbalanced by the inclusiveness of *world;* in 38 the intimacy is reinforced.

These responses show a semantic constant holding across a great variety of performance variables. 'Closeness' can be thought of in many ways: in relation to physical distance, to a 'you' in contrast with a 'he' and a 'he' in contrast with an 'it', to members of one's own group as against outsiders, and to other forms of psychological distance. Even the greater formality of *-one* is not a totally separate feature: it may be related to considerateness; there is little occasion to be formal with people in the mass.

The subjects were invited to write down the principles which they thought might have guided them as they answered the questions. Seven of the twelve did so. One gave a virtual paraphrase of the hypothesis which I had already set up: "[-one] intimacy, definiteness, individuality; [-body] distance, indefinite reference, collectivity: i.e., if a context seems to presuppose semantically the above co-occurrence, then [-one] or [-body] is chosen to agree with that context." All seven remarked the greater definiteness or individuality of *-one:* as another student worded it, *"Anybody* implies a certain air of abandon, as if you do not really consider the identity of the person to whom it might apply."

The similarity between *one* and its compounds has to be more than a coincidence. If we try to imagine a white racist saying the following sentences,

> Once one is charged with raping a white woman, one hasn't a chance.
> Once a person is charged with raping a white woman, he hasn't a chance.

the first is just about impossible, because the speaker identifies himself with the alleged offender. The second is a little out of character because it implies sympathy, but it is not incongruous. Whether the tie between *one* and *-one* is close enough to call them identical is something that a lexicologist would have to decide, to fit the delicacy of his analysis, but it would be hard to deny that they are semantically related.

As a suggestion for future study, it is worth mentioning that other couplets of indefinites may well display similar contrasts in meaning. There are, for example, *-where* and *-place (somewhere, someplace, nowhere, noplace,* etc.), *-how* and *way (somehow, some way), -time* and *-day (sometime, someday),* and doubtless others. It would be interesting to discover from a semantic standpoint what happened to **somewhen* and **somewho,* why no *somewhy* developed, and how *somewhat* diverged from *something.*

FOOTNOTES

[1]My thanks are due to Wayles Browne for suggesting the *-one/-body* question and to Professor Einar Haugen and members of his Harvard class (Introduction to Sociolinguistics) for their cooperation in the test (see Table 1).

[2]The separate figures are: *anybody* 192, *anyone* 552; *everybody* 390, *everyone* 953; *somebody* 179, *someone* 908.

[3]As we might suspect, Scots and Scottish English, where *body* survives as an independent indefinite pronoun, have a pronounced preference for the *-body* compounds. J. D. McClure writes (personal communication), "To me, *someone* and *anyone* are definitely foreign." As for *one, "a body* or

you are the only indefinite pronouns a Scots speaker would use. To me, *How is one to tell the difference?* could only be said facetiously or sarcastically: I'd say, depending on the degree of formality, *How are you to tell the difference?* or *What way is there of telling the difference?*".

[4]Echoed by Poutsma (1916: 1302), also by Curme (1935: 18). Mincoff (1958: 282, 288) mentions only the greater colloquiality of *-body*. Kruisinga (1932: §§343-4) notes the exclusive use of *-one* with partitives and the absence of *each body* (as against *each one*), which points to the greater generality of *body;* but he fails to make a clear distinction between the *éveryone* type and the *èvery one* type.

[5]Curme 1935: 18 calls attention to the "exact equivalence" between the older sense of *body*– incorporated in the *-body* compounds– and that of *person.*

[6]Poutsma chides Jespersen here for seemingly having overlooked "the fact that in this position *one* is a substitute for a preceding or subsequent noun (often implied in a pronoun), i.e. as a prop-word of the second kind, a function in which *body* is never employed" (1302). This may be so in a historical sense, but Jespersen was right in regarding *-one* as at least potentially the composite indefinite pronoun. The spelling fails to show that he probably intended *éveryone,* not *every óne. Everyone of my friends* is not the same as *every one of my friends.*

[7]Restrictions also do not apply with non-partitive uses of *of*. Indeed, it appears that partitiveness is restricted to quantifier plus *of,* in construction with the inclusive noun. If something happens to loosen the connection, the notion of partitiveness is weakened. This is particularly noticeable when the *of* phrase is fronted, becoming then rather like an absolute:

*Someone (*somebody) of the population can surely be expected to volunteer.
*Of the population someone (somebody) can surely be expected to volunteer.
?Someone (?somebody) of that entire population can surely be expected to volunteer.
Of that entire population someone (somebody) can surely be expected to volunteer.

The last example resembles 'Given that entire population, someone etc.' There is an underlying predication in the added demonstrative and quantifier: 'Since there is that population and it is so large'. The intensification helps by widening the range (see the next paragraph in the text). Similar examples:

*Of my friends nobody is quite like her.
Of all my friends nobody is quite like her. ('I have many friends and....'
*Somebody of the group, I was sure, had the secret.
*Of the group, somebody, I was sure, had the secret.
Of the group at my house, somebody, I was sure, had the secret. ('There was a group at my house, and....')
?Only one of the force was ever captured.
?Only one of the invading force was ever captured.
Of the invading force, only one was ever captured. ('There was an invading force, and....')

The *-body* compounds have to overcome the more powerful restriction: any suggestion at all of a partitive. *One* is acceptable with a true partitive. Hence the different degrees of acceptability in these examples. On the underlying predication, see Bolinger (1972: 132, 303).

[8]Jespersen dismisses his counter-example *She's nobody particular* as an instance of 'a person of distinction or rank [or the opposite]' (§8.441), i.e., 'She's a nobody'. But the meaning is broader than that: the speaker is saying that he doesn't care who she is, covering identity and incidentally rank (compare the rank-conscious question *Do you know who I am?,* basically identity). There is a lack of closeness and individualization.

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THE METAMORPHOSIS OF ALLOMORPHS

Raimo Anttila
UCLA

A characteristic aspect of the 1970s has been a return to "concrete" phonology (under a multitude of forms, or schools). This movement has of course necessarily entailed a resurrection of morphology as a component of more direct independence than assumed in the 1960s. A natural breeding ground for "phonological" difficulties was of course the allomorph relation (or situation). For the American structuralists the state of affairs gave reason for the item-and-arrangement approach, for "generative" grammarians the solution was an extreme item-and-process mechanism. In other words, the basic notions in these opposing positions are suppletion vs. underlying invariance. On the whole allomorphic variation has been felt as unnecessary complicating redundancy, and linguistic training has been basically trade schooling in how to strip off inelegant variation from descriptions. Now, however, some forms of semiotics (French in particular) have defended a semiotics of redundancy or variants. Furthermore, Shapiro (1974) defends the actual variants in terms of morphophonemics as semiotic, or semiotic theory of markedness. On this basis I myself looked into the indexical function of allomorphs (Anttila 1975). Allomorphic alternation enhances the unity of the total word (as has been pointed out by Sapir, among others). If allomorphs have independent function, as seems to be the case (Anttila 1975), the suppletive aspect gets strong support. But a new problem arises: how to define allomorphs, or for that matter, morphemes? This is an important question, since allomorphs are so central in lexical structure. However, I do not attempt any definitions here, but treat the characteristics of the allomorphs in terms of their dynamism.

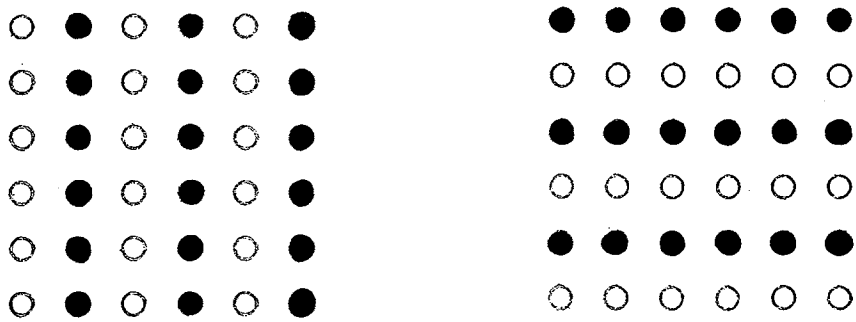
In principle the allomorph relation is parallel to the dialect relation. Here also, two opposing positions have been competing for the status of ultimate truth: Each dialect needs an independent grammar of its own (cf. the independent allomorph) vs. each dialect is derived from the same underlying grammar (cf. the invariant underlying morpheme). St. Clair (1973) showed convincingly that both positions are unable to explain the known facts about language and its use, and suggested instead that the dialect relation be looked upon as a case of family resemblance in the Wittgensteinian sense. This

is indeed the best solution to date; dialects are now complex networks of overlapping similarities and differences so that there is always enough agreement for interpretation. I extended this solution to the allomorph situation (Anttila 1974). We gain philosophical or conceptual justification or understanding of the allomorph relation, even if all the problems of defining similarity remain. But that cannot be helped; it is a necessity, the basic axiom.

This was roughly the situation in the summer of 1974 (as it appeared to me). Then Pike (1975) reminded us of his morphological analyses of the (early) 1960s and Slagle (e.g. 1975) resurrected the viable parts of gestalt psychology as a foundation of the semantic structure of a natural language. And interestingly enough, also St. Clair (1974) found help for phonology from gestalt psychology. All these works take a critical stance against generative grammar. Neisser (1967), however, believes gestalt psychology to be analogous to structural linguistics, the highest form of which would be Chomsky's transformational grammar. This last point is a wrong analysis or equation, since the two main targets of Köhler's critiques were behaviorism and introspectionism. Note that Köhler was not against introspection (like the behaviorists), but introspectionists who devised elaborate techniques by which to exclude the surroundings or environments from the observer. This way all factors of learning would be excluded and pure Dinge-an-sich would appear without the distortion of education (or cultural context and conditioning). This aspect is rather a direct counterpart to Chomskyan innatism. When we further realize that generative-transformational linguistics is a clear form of behaviorism (see particularly Itkonen 1974), we see that generative grammar represents basically those aspects that Köhler argued against, and convincingly indeed. One can also note that Slagle's work (or position) is a rejection of just these two factors: behavioristic American structural linguistics and transformational-generative grammar. But since <u>all</u> linguistics is now structural linguistics, Neisser is <u>unwittingly</u> right in the sense that viable structuralism <u>should</u> be linked with gestalt psychology. This is indeed <u>what</u> Slagle has started to do (and also St. Clair); before him one saw only stray references to gestalt psychology in the framework of French structuralism as a doctrine of much wider purport than linguistics alone. Such references are indeed quite justified, since structuralism (linguistics included) is based on the networks of similarities and differences; and the whole is more than the sum of its parts. It is in fact curious that linguists have not wanted to use the experimental evidence gathered by the gestaltists to further their own structuralism.

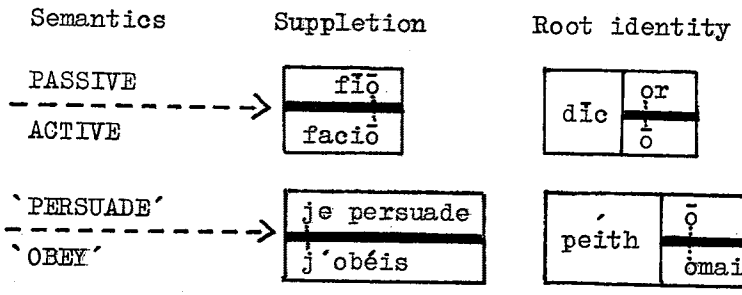
Somehow introspective speculation turned out to be much more attractive (it is after all easier to 'prove'). One also senses that Neisser's attitude toward the gestaltists is that they had too easy an answer to perception and cognition: similarity. Whatever its problems are, it is indeed the basic factor we need for the structure of language (Pike, Slagle, St. Clair, Anttila).

Similarity is a primary factor in the structuring of stroboscopic movement where we see movement although we know that the stimuli are stationary. This is direct evidence for the dynamic force of similarity. A well known case is also the patterning in the Wertheimer dot configurations (see e.g. Köhler 1940:61-62):



Here equidistant dots make either vertical or horizontal patterns in which the same principle of similarity operates as in stroboscopic movement. I propose that in linguistic morphology, or paradigms in their widest sense, we have exactly the same factors, in short: <u>morphology is analogous to a complex case of stroboscopic movement or Wertheimer dot patterning</u>. Although the recognition of gestalten within morphological structure is a complex matter, we still seem to have Wertheimer's five principles at work: (1) <u>proximity</u> (stimulus patterns which appear close together are likely to become unified into the same gestalt), (2) <u>similarity</u> (stimulus patterns which resemble each other tend to become unified), (3) "<u>common destiny</u>" (stimulus patterns that move together or change together are perceived as part of the same thing or as belonging together), (4) "<u>good gestalt</u>" (forms that are not quite regular tend to be perceived as more regular than they are), and (5) <u>closure</u> (forms which are incomplete tend to be perceived as complete). Basically principles 1 and 3 cover contiguity, 2 and 4 similarity, but all provide open-ended principles as needed for natural language, and all agree thus with the notion of family resemblance.

Identity is of course the extreme case of similarity, and in languages which have no allomorphic alternation indeterminacy is shifted to the word level. Words that share the same morphs are now partially similar (see Anttila 1974, 1975). Given the communicative context with its various semiotic axes the proximity factor can totally break down, in other words, the same form can be pulled into two (formally identical) words, as is well known from polysemy and homophony. Thus identity can be broken by the influence of the semantic ground, the total (sub)field (in the semiotic sense). Now, we end up with situations like cold `Erkältung, Schnupfen´ and cold `kalt´, or Schloss `castle´ and Schloss `lock´. When these forms occur in different fields there is no reason or necessity to perceive any similarity or identity. This is also true of compounding structures like German Fahrstuhl; this is not perceived as going with fahren or Stuhl, it is just a unit `elevator´. In short, even with morphemic identity words can shift apart, and this is of course easier with allomorphic differences (e.g. bake/batch, bleak/bleach, etc.). The converse is also well known and in fact commonplace, to just take an example at random (modified from Engler 1973:39):



In these diagrams (for Latin, French, and Greek) certain similarities do cross the horizontal lines of difference (e.g. Latin and Greek -ō-, French j(e), and note the accidental similarity in Latin f´s), but these pictures show the main idea quite adequately. The point is that suppletion is perceived as similar in spite of the formal difference, because the ground is the same, in other words, the forms have common destiny, they move together, and are perceived as similar through the working of contiguity. Thus it is no wonder if allomorphs are also perceived as similar or same, since here we have proximity (1), similarity (2), common destiny (3), good gestalt (4), and even closure (5). The allomorph situation looks thus as the most general or strongest gestalt patterning partaking of all five principles.

It is now useful and necessary to return to Pike´s work on morphology with indeterminate segmentation (1963,

1965, 1975). Pike's basic requirement here has been that grammar is both a particle (unit), a wave, and a field phenomenon (cf. word-and-paradigm), and thus the linguist needs the freedom of shifting between these aspects. And of course the language user needs the same flexibility. A field `structure' on a lower level can be a particle on a higher level, and this is indeed basically what structuralism has striven for, although often with considerable derailment. For the allomorph situation we see that strict surface phonology gives us the particles, morphophonemics the waves, and morphemes in paradigms the fields. Note how the wave effect of morphophonemic alternations come out directly with full force in Shapiro's conception of markedness reversals, which are guided by a semiotic theory. This is a good indication of actual waves through the morphological maps. Now, obviously larger chunks like allomorphs must be some kind of minifields within their morphemic limits, and particles in the larger paradigm. Pike has himself drawn attention to the optimality of "suppletive" forms in a field, e.g.

king	queen	vs.	prince	princess
stallion	mare		lion	lioness

and so on (1963:23, fn. 17). Suppletion is also extremely common among copulas and other basic vocabulary (e.g. plural formation, adjective comparison, etc.). Pike calls the suppletive patterning ideal, the agglutinative type simple (1963:18, 1965:204):

IDEAL

a	b	c
d	e	f
g	h	i

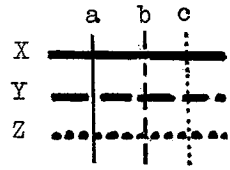
SIMPLE

p	t	k
b	d	g
m	n	ŋ

In the simple pattern we have similarity vectors that stretch over longer axes, in the ideal pattern independent particles. In the simple pattern each particle partakes in two similarity vectors, i.e. the particle is an intersection of two features. The diagram is based on a typical inventory of consonants, and the vectors are what Bernard Bloch called isomeric sets. Pike wanted expressly to see if in grammar there might be an analogue of a phonetic chart, and the simple pattern does indeed represent the basic patterning of "agglutination" (1963: 16, 1965:204):

Xa	Xb	Xc
Ya	Yb	Yc
Za	Zb	Zc

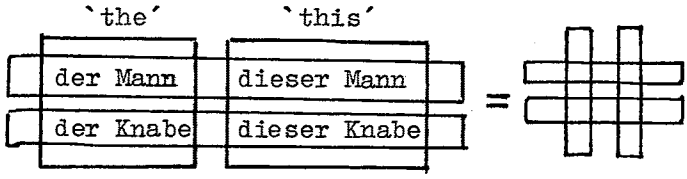
=



To give a simple example for this grid let us organize Finnish kana `chicken´ (X), puna `red color´ (Y), kilo `kilogram´ (Z), -na essive (a), -sta elative (b), and -lle allative (c) accordingly (with segmentation indicated):

kana-na	kana-sta	kana-lle
puna-na	puna-sta	puna-lle
kilo-na	kilo-sta	kilo-lle

The same is true of phrases with "free" words (Pike 1965:196):



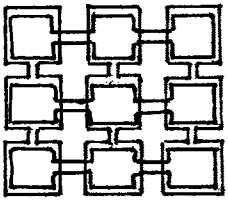
This kind of matrix field structure can be extended into syntax, which comes close to what Bloomfield thought about partially alike and partially different structures in syntax and other domains.

The conventional morpheme turns now out to be a special instance of a matrix formative or vector pattern (Pike 1963:16, 1965:203):

> a formative which occurs in every cell of a particular vector (row or column) but not in any other cell. Conventional morphophonemic writing, from this viewpoint, becomes a device for representing a set of formatives which are complementarily distributed in a row (or in a column) by the same symbol even though they differ in phonology. It creates, that is, a pseudo-simple vector formative (1965:203).

Elimination of variation or redundancy is now an orthographic convention only, made for the sake of simple presentation. Note that suppletion of the single cells in the ideal paradigmatic map is in fact the most efficient type in that it enhances direct anchoring to "real-

ity" or experience. Pike phrases the oscillation between simple and ideal patterns (in linguistic change) as a kind of paradox: "maximum irregularity of formative (or morphemic) pattern leads to minimum complexity of field structure" (1963:18). We see now that the rise of allomorphy is a move toward the ideal pattern. Here a vector assimilates to a crossing vector and produces a more ideal sign (this is treated in Anttila 1975 from the point of view of index formation within words). This is the midpoint in the oscillation and thus the typical situation, a pattern that is both simple and ideal, and I would draw the diagram for it this way:



TYPICAL ALLOMORPH PATTERN

Here the links can be either easily broken or straightened out to simple vectors.

Pike's terms would at first blush seem to be curious, but they make perfect sense. Note that generative phonology which leveled out actual alternants was/is extra pseudo-simplistic in this sense. Moreover, there had been warnings all along that the morphophoneme is a fiction. Generative phonology, however, elevated a fiction, or orthographic convention, into a theory. This went against the well established fact that language is a system of relations, and Linell (1974) in fact points out that generative phonology is based on the category mistake of making a relation into a thing.

St. Clair (1974) seems to think that generative phonology could be rescued by rejecting the polarity principle (that contrasts in content should be directly represented by means of contrasts in expression). It seems that Hjelmslev and stratificational grammar have best avoided this pitfall, although St. Clair takes to "certain recent developments in generative phonology which demonstrate that `distinctiveness' cannot be characteristically associated with phonological contrasts." Instead, he argues "that the cognitive basis for distinctiveness can only be due to what psychologists have come to call "perceptual constancies" (Koffka, 1935)." Native speakers react to perceptual contrast, not minimal pairs, and this should work also in the "perception of the underlying segments and forms within the theoretical framework of generative phonology":

> If a phonological segment is modified in the course of a derivation, it will not be perceived as another sound, even if its modification results in minimal pairs, or in the complete overlapping of the allophones of disparate phonemes.
>
> ... minimal pair drills in the language laboratory results in failure. The acquisition of communicative competence requires that the student be taught lexical alternations, as it is this kind of information that he must use in ascertaining this new set of phonological constancies before him. Minimal pairs only provide information which is partial and misleading (quoted from MS).

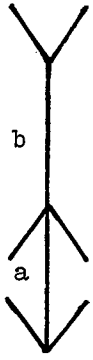
St. Clair's gestalt argument is very important, also because it is independent of Slagle's work (where all the main figures of the movement are referred to). But the two quotations seem to be contradictory. The generative strategies of process and order have obfuscated rather than elucidated the complexities of morphophonemic structure (Shapiro 1975). Furthermore, the requirement of the primacy of lexical alternations contradicts the very idea of underlying invariant sound segments. In other words, the principles of gestalt patterning do not support 'derivation' away from invariance, but production and movement toward a good pattern (principle 4 above). When one learns lexical alternants or any other variants, it is the whole that determines the parts. Thus cases like electric/electricity (with k/s) have a "common destiny", and this creates the constant features one perceives. The holistic aspect is crucial, i.e. the allomorphs, rather than sound units. In fact, the old school-grammar kind of principal parts display approach is theoretically quite good, contrary to what is generally assumed (Pike, Linell, Anttila 1974:335). One must add, however, that this point has been appreciated in England where the word-and-paradigm aspect never died completely out. This can be seen in e.g. Matthews (1974), which in addition presents a sandhi-based model of allomorphic relations, although it does not discuss the tradition treated in this paper.

These short comments have dealt with the perceptual component of reality (see Shapiro 1975: Fig. 3), or the stretch between perception and conceptualization. Shapiro stresses the fact that conceptualization must be followed by evaluation, and this is what markedness does, since it represents the form of meaning. His work deals with the formal coherence between variants, and this is indeed crucial. My comments pertain to the substance of linguistic form. Shapiro deals with the function of alternations in grammar. One should not explain away surface alternations, and relationships cannot be repre-

sented as processes. Shapiro discusses the interpretant corner of the sign, I deal with the substance or medium (Mittel). Not only is a change of ground crucial in interpretation (e.g. in metaphor, which is basically changing the ground of the sign till it makes best sense), but we see the same for forms (substance). The same sign (e.g. cold) need not be perceived as same if the grounds are different enough, and different forms can be identified if the ground is right (suppletion). The more similarity between themselves the allomorphs display the easier they go together. Allomorphy enhances the total gestalt of the word (cf. Anttila 1975). In a way it camouflages derivation and emphasizes dictionary entries. Unfortunately it is not possible in this context to reproduce the various gestalt figures which show various assimilation and dissimilation effects. It is this framework that substantially adds to the family resemblance aspect in that we get empirical psychological justification for the philosophical necessity. Whatever is real in linguistic structure can be used for artistic purposes: Similarity as a crucial factor in combining form is seen also in rhyme and alliteration. Thus also in allomorphy similarity ties together variants with "themselves".

Even Pike's principle of matrix permutation (1963: 4) is in fact similar to gestalt patterning in that here one tries to find out, on purpose, the most attractive configuration. Once found, this pattern is taken as revealing the "realities" of morphological structure.

And one more example of field influence. The strongest field effect can be experienced in the (adjacent) Müller-Lyer illusion, in which the adjoining planes (fields) influence the perception of the lines a and b, equally long. Linguists have ignored gestalt theory as irrelevant because they have misinterpreted the experimental data on brain damaged patients. These findings have been almost universally interpreted as showing that the language dominant hemisphere processes sensory information in an analytic manner while the right hemisphere does it in a holistic (gestalt) way. This interpretation, however, is extremely insecure, and in fact Slagle (forthcoming) shows that the language dominant hemisphere is more susceptible to the Müller-Lyer illusion; the 'left' hemisphere is more holistic than the 'right'. Slagle further notes that it does indeed make sense for the field effects to be stronger in the language dominant hemisphere, since the very process of categorization requires unification of disparate instances of given concepts. The stronger field effects in the left hemisphere help for the same reason that they hinder in



the case of recognition of faces - similar patterns are more likely to become united.

Whatever the endpoint in this line of argument will be, our understanding of the allomorph relation is certainly now under a metamorphosis. It shows further a telemorphosis toward a better solution.*

* In this context I have to omit an earlier blossoming of gestalt concepts in morphology. In Finland Eliel Lagercrantz looked at Lapp consonant gradation in this vein, and part of his theory lives on in the work of Paavo Ravila and Erkki Itkonen. It should also be noted that Terho Itkonen has used gestalt terminology (hahmottua) in connection with syntactic patterns (since 1956 at least). Quite recently again, the term 'gestalt' figures in Holman (1975), and we are back in Lagercrantz's domain. I hope to treat these matters on another occasion. Now, however, Philip Baldi and Uhlan Slagle deserve my thanks for useful comments on the present paper.

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DOES WORD ORDER IN NOUN COMPOUNDING REFLECT SENTENTIAL SYNTAX?

Greta D. Little
University of South Carolina

Perhaps the real question is how do noun compounding patterns reflect sentential syntax? If we accept a generative source for compounds, as "noun-like versions of sentences" described by Lees (1963: 54), we must accept some connection between word order in noun compounds, and word order in sentences. But the question remains: At what level does this connection exist?

One possibility is that the connection between compounds and sentential order is historical. Such a diachronic explanation has been suggested by Talmy Givón who claims the noun compounding pattern must have arisen at a time when such a pattern characterized sentences of the parent language (1971).

Givón's approach to compounding is especially useful because it provides information to linguists seeking evidence of syntactic change not attested in written records. For example, Amharic, the chief language of Ethiopia, is thought to have undergone vast syntactic change. One of the hypothesized changes is Noun: Genitive order to Genitive: Noun order. The modern construction consists of a prepositional prefix <u>yä</u> - marking the possessor which procedes the noun.

1. yäsäwəyyew : bet
   gen-man-the : house
   the man's house

Evidence of the previous NG order can be found in noun compounds like (2).

2. betä : mängəst
   house-acc : government "palace"

Comparative Ethiopic evidence tells us the - <u>ä</u> is the accusative ending, and that the construction is the accusative construct state of Ethiopic.

This construction was used to express the genitive. Examples like (2) provide evidence of NG order in early Amharic. The accusative marker, -ä makes it clear that this compounding pattern is a relic of an early stage of Amharic, a stage when the noun/genitive order was NG.

It is also possible to take a synchronic approach to explain compound orders which are parallel to the basic patterns of a language. W. P. Lehmann objects to a strictly diachronic explanation because he objects to the assumption compounds are particularly archaic (1969: 2). He argues that there is a direct relationship between compounds and basic syntax only in productive compounding patterns (1974: 76). The use of compounds in the investigation of early syntax is also a part of the synchronic approach. Lehmann has shown that the most common PIE compounds had OV order. This order corresponds to the OV order indicated for PIE by other typological principles (1969, 1973). English verb/object compounds offer promising ground for evaluating the two approaches. We can compare their ability to explain the fact that English has productive compounding patterns with both OV order and VO order. Givón's own presentation of the diachronic approach is limited to examples of OV compounds in English: lion-killer, house-cleaning, clambake, mouse-trap, farm management (1971). On this evidence, Givón concludes that English was once an OV language, citing Lehmann's work on the OV structure of PIE for confirmation of his findings. Givón does not treat any of the VO compounds, so he offers no explanation for the contradictory patterns.

The problem with Givón's conclusion is the chronology. He is using syntactic order as the basis for compounding patterns which began to emerge at the end of the Old English period. Surface sentential order of English at that time was almost evenly distributed between OV and VO (Fries, 1940: 200). According to Lehmann, even late PIE had begun to show signs of moving toward VO order (1969). Although the diachronic approach can be useful, as it was with Amharic genitive compounds, it can produce unnecessarily speculative conclusions. Comparative or historical evidence to confirm that the basic syntactic patterns existed contemporaneously with the compounding pattern is an important part of the diachronic approach.

Lehmann eliminates the chronological problems by suggesting that the connection between sentence and compound order applies only to productive compounds. Non-productive compounds may be said to reflect the syntactic order which existed when the compounding patterns emerged. If this connection is valid, we would expect to find VO order in currently productive verb/object compounds in English. However, the most productive verb/object compounds are object nouns followed by verbal derivatives (see Quirk and Greenbaum, 1973). "Taxpayer" and "sightseeing" represent the two most productive patterns. Contrary to expectation, they do not reflect contemporary word order.

A possible explanation is that the compounds are OV because the deep order of English is OV. However, in the absence of any independent motivation for such a claim, and in light of the general lack of

agreement on the question of order in the base component, OV deep order is not a viable explanation for these compounds.

A more reasonable explanation is that these OV compounds do not reflect the verb phrase order of English sentences because their derivation includes a further stage. When a verb phrase is nominalized the verb becomes the head word of the new noun phrase, and the object becomes a modifier of that head word. Once such an analysis is possible, the stage is set for a switch in word order since the dominant noun/modifier order for English is modifier: noun. This switch is not mandatory since VO compounds do exist. The simple verb + object pattern ("pickpocket") is no longer productive, but the verbal + object pattern ("whipping boy" or "reading material") is still fairly common in contemporary English.

Let us examine noun compounding and such reinterpretation in languages which exhibit different word order patterns from those in English. I have chosen Amharic and Swahili because they are languages which I know well enough to vouch for the data. Comparable Amharic compounds consist of an object and an agent derivative of the verb. The order is OV, as expected in an SOV language.

3. kəfu : säri "evil-doer"
evil : worker

Swahili has a single compounding pattern, verb derivative : object.

4. mla riba "usurer"
person
prefix + eat interest

The VO order of the compound corresponds to the SVO order of Swahili.

If nominalization in Amharic and Swahili were accompanied by the kind of re-interpretation suggested for English, what would be the result? In Amharic the verb of the OV compound would become the head word and the object its modifier. Word order would not change, for modifier: head is the pattern for modifiers in Amharic. If a Swahili verb phrase is nominalized, the resulting noun phrase will have the head word (former verb) first with its modifier (former object) following the noun. This is the modifier/head order found throughout Swahili. The compound may be reinterpreted, but it retains the VO order.

In terms of word order typology, both Amharic and Swahili are consistent with respect to verb/object order and modifier/head order. English, on the other hand, is not. SVO languages (like English) usually have head: modifier order (Greenberg, 1966; Lehmann, 1973). Therefore in consistent languages we can expect a single compounding order for verb phrases, because reinterpretation of the phrases can not affect the order. Inconsistent languages such as English may have either or both orders as a result of reinterpretation.

The possible distribution of verb/object compounds can be formulated according to verb/object order and modifier/head order. Such a typological explanation can allow us to explain the data which do not reflect surface order patterns without losing the advantages of a formal connection between order in compounds and sentences.

VO HM	1 type compounding possible	VO
VO MH	2 types possible	VO OV
OV MH	1 type possible	OV
OV HM	2 types possible	OV VO

Another typological generalization about verb/object compounds has been proposed by Solvieg Pfleuger who suggests that such compounds can occur only in languages which have a predicate node, i.e. where the object and verb are contiguous. However I question such a claim in the context of traditional transformational grammer because the T-component is so powerful that it can easily move constituents into contiguous positions as part of the derivation. In addition verb/object compounds do exist in at least one VSO language, Maasai:

5. l-oo-uro-kutuki "dentist"
   one-who-cuts mouth

Pfleuger's claim is also questionable because of its implications for verb/subject compounds. They rarely share a node other than the primary S node. Yet in English there are many examples of both SV and VS order in compounds: beesting, flashlight, cleaning lady. Furthermore even in SOV languages where the constituents are not contiguous, subject/verb compounds exist. Such compounds occur in Vei, for example.

6. día - mo "lover"
   love person

7. tere - kari "day break"
   day break

Neither Amharic (SOV) nor Swahili (SVO) have verb/subject compounds. A connection between consistent patterns and compounding seems an unlikely explanation for verb/subject compounds. Also verb/subject compounds do occur in Turkish, a language with consistent patterns. There seems to be no grounds for explaning the occurance of verb/subject or verb/object compounds on the basis of word

order typology. Many verb/subject compounds are derived from relative constructions:

"cleaning woman" < woman who cleans

In both Amharic and Swahili the only morphology available for verbal elements in connection with subjects is the relative form:

6.	Amharic:	yammialaqsəw : ləǧ rel-cry : child	"child who cries" or "crying child"
7.	Swahili:	mtoto aliaye child he-cry-rel	"child who cries" or "crying child"

Relatives in Swahili and Amharic can not occur in reduced form. The relative morphemes are bound to the verb forms. Thus morphological restrictions also play a role in the formation of compounds.

Let us return to the original question: Does word order in noun compounds reflect sentential syntax? We want to say yes, because if there is such a connection, compounds can give us valuable information about earlier syntax. However, even a cursory examination of English alone makes it clear that there are no pat answers. The connection between prior syntax and some compounding patterns has been shown repeatedly. The development of compounds from existing patterns is likewise evident. The VO pattern of "pickpocket", "makepeace" emerged in the 1500's, about the same time that VO order became firmly established in written English (Fries, 1940: 200). Yet today the dominant pattern is OV, as evidenced by the creations of NASA: "Launching the vehicle" becomes "vehicle launch" (McNeill, 1966).

A typological approach such as outlined above, combining modifier/head patterns and verb/object patterns can provide an explanation for the variety of patterns in English, but the validity of such an explanation rests ultimately on its power to explain compounds throughout language.

But it cannot explain why certain types of compounds do not occur in some languages. This question will eventually lead us to the heart of how morphology and syntax interact.

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FROZEN ADJECTIVE-NOUN COLLOCATIONS IN ENGLISH

Ulf Bäcklund
University of Umeå, Sweden

Abbreviations

ALD	*Advanced Learner's Dictionary of Current English*
BC	Francis, W.N., *A Standard Sample of Present-Day American English, for Use with Digital Computers*. Brown University. (Generally referred to as 'the Brown Corpus')
H.	*Harrap's Standard French and English Dictionary* Part 2, English-French
W.	*Webster's Third New International Dictionary*
WS	*Webster's Dictionary of Synonyms*

1 *Collocation* might be defined as the co-occurrence of lexical items. In this paper the concept is used in a restricted sense: the co-occurrence of adjectives and nouns in a headed construction. In order to clarify what I mean and to explain some terms that will be used in the discussion, let me give an example. In a collocational study of the adjectives *strong* and *powerful* in a certain limited stretch of text, the following pattern has emerged:

strong: *tea* 4 argument

powerful: *car* 4 argument

Thus, in the text examined, *strong* modifies *tea* four times and *argument* once, whereas *powerful* modifies *car* four times and *argument* once. *Strong* and *powerful* are the *nodes*. *Tea* and *argument* are the *collocates* of *strong*. The two nouns constitute the *range* of *strong* in this limited text, while *car* and *argument* constitute the *range* of *powerful*. *Tea* is the *main collocate* of *strong*, and *car* is the *main collocate* of *powerful*.

1.1 The discussion in the present paper is based entirely on an informant investigation undertaken 1974 in Great Britain. This was carried out by means of a questionnaire, in which were listed a large number of adjectives whose ranges were suspected to be rather restricted, or even conspicuously dominated by a single lexical item (an extensive text-material had been used as a pilot-study). The informants were asked to write down the noun which they most closely associated with the adjectives in a modifier/head construction. This was followed up by personal interviews where the collocations which were significant for my purpose were discussed. A few nouns were also included in the investigation. Number of informants: questionnaire: 60; interviews: 30.

2 (1) Happy riddance!
(2) You arrant hussy![1]

Native speakers of English would find these sentences somewhat odd, (1) more so than (2). How is this oddity to be accounted for? In neither of the sentences is there a violation of lexemic syntax or sememic syntax in Lamb's sense. **Him riddance good to* is ungrammatical (violation of lexemic syntax), and **Bad riddance*[2] is nonsensical (violation of sememic syntax). Both (1) and (2) could be said to be unlexical, representing a violation of the structure of lexis. Both (1) and (2) would be perfectly understood by the hearer, but an additional message is inherent in the two utterances: the speaker does not 'know' English properly or he deliberately avoids the 'natural' adjective, which blunts the relief expressed by (1) and the denunciation expressed by (2).

3 Firth says in his oft-quoted remark:

> "Meaning by collocation is an abstraction at the syntagmatic level and is not directly concerned with the conceptual or idea approach to the meaning of words. One of the meanings of *dark* is its collocability with *night* and one of the meanings of *night* is its collocability with *dark*."

The purpose of the present paper is to show that many firmly established collocations *do* have an effect on the conventionalization of the concepts of their participants. There is a semantic overflow, either unidirectionally or bidirectionally.

4 In the three collocations with nouns as nodes discussed below there is complete monopolization, that is, the range of the noun is constituted by a single adjective. This adjective monopolizes the noun and is, according to my informants, *obligatory*.

4.1 Bolinger (1972) says that *well* in *well-conceived plan* and *the case was well argued* has the 'approval' meaning (contrary to expressing degree as in *well-traveled*), adding in a footnote that this is equally true of *well* in *we are well rid of them*, since *rid* is a non-degree word and similar to a past participle. However, *conceived* and *argued* are entirely different from *rid of*. This difference is manifest on two levels:

(1) Syntax

With *conceived* and *argued*, *well* is gradable: *very/extremely* etc. *well*. Predication is possible: the conceiving of the plan/the arguments was/were good. With *rid of*, however, grading and predication are impossible. Thus *well rid of* is syntactically 'frozen'.

(2) Semantics

Well argued can be transformed into *argued in a good way*. Thus *well* expresses manner. *Good arguments* constitute a class of arguments: those that serve their purpose well. From this follows that substitution can take place: we can have *good/excellent/bad* etc.. *Well rid*, however. is a semantic unit expressing something like 'satisfaction', 'relief'. Let us consider this in an example from my text material:

I think Winnie is well rid of him.

This can be paraphrased: she will do better without him/it is a relief for her that he has left her. The concept expressed by *rid* is 'separation' with the addition 'good'. The function of *well* is the same as that of *perfect* in *perfect gentleman*, to underscore or repeat the positive concept inherent in the noun: 'good good separation'. The communicatively important feature is this evaluation. The idea of 'separation' is implicit in the situation, something already known to the hearer. The contrary is the case in:

I think she is rid of him now.

The message is here the idea of 'separation', the paraphrase being: he is not with her now, which, as we know, is good for her. *Good riddance*, used exclusively in exclamations, is a manifestation of the concept 'relief'; the idea of 'separation' is always implicit.

I have actually found one instance of *riddance* used independently (BC):

He successfully campaigned for the riddance of Death Avenue.

This was flatly rejected by all informants, however. In addition to "an act of ridding, freeing, or cleaning", W. records a secondary sense of *riddance*: DELIVERANCE, RELIEF. This is obviously misleading. Only the combination of the obligatory collocate of *riddance* and the node itself expresses this concept. The semantically redundant adjective *good* has given rise to a secondary definition of the noun.

4.1.1 A collocation rather similar to *good riddance* is *a good mixer*, used about a person who is "at ease with others on a social occasion" (ALD). *Mixer* is the agentive of the verb *mix (with)*, 'get on well with other persons', which is a desirable quality, reflecting an instinct in human nature. Thus the noun is apt to be positively coloured. It attracts a + value. This 'marker' is underscored by *good*. Thus *good* is semantically redundant but is, *for this very reason*, attracted by *mixer*. All informants stated that, when referring to a person lacking this desirable quality, they would say *not a good mixer*. They felt that *bad mixer* was 'unnatural'. This shows that *mixer* is not apt to lose its markedness, as is the case with *inspect* which might drop its component 'with care'. *Inspect carelessly* is perfectly normal. Thus the component 'with care, carefully' is not as firmly established as is the + component in *mixer*.

The explanation for this is probably that *mixer* reflects a universal feature, denoting a person inherently equipped with a desirable quality, whereas *inspector* has a constructive definition (Bolinger 1965), i.e. it denotes a person who is supposed to examine something carefully.[3] Of course, he can fail to do so, performing his duty carelessly.

4.2 The manner in which I began to grasp the meaning of *high dudgeon* may shed some light on a fundamental principle in semantic redundancy. The informant who mentioned this phrase as an example of a 'fixed' collocation tried to illustrate its meaning by giving a sentence in which it occurred. In it he referred to a person whom I knew quite well: "If her children said ..., she would walk out in high dudgeon". This is what I pictured in my mind: she turned on her heels, walked out of the room with quick, determined steps. Her cheeks were flushed. She felt righteous indignation. But in a few minutes her resentment would vanish and she would take a more objective view of what had occurred. The justification of her fit of anger would not be self-evident any more. She would forgive both her children and herself (for acting rashly). A more formal analysis of the meaning of the phrase such as I grasped it would contain the following 'components':

1. indignation, 2. action (walk), 3. visible (walk, high colour), 4. short-lived, 5. typical of a hot-tempered but kind person.

This 'componential analysis' is evidently too elaborate, i.e. there are too many 'components', because it is based on a single situation and because I know the person showing this reaction. Hence my interpretation is affected by the qualities which I ascribe to this person (especially component 5.). All the other informants confirmed that *high dudgeon* occurs in the context *walk out in*. Thus my first unrefined analysis remains fairly intact. Besides components 1. and 2., which are obviously correct, the other components remain more or less because of the basic concept 'action', 'non-concealment'. Such a reaction to an offence suggests flushed cheeks, quick temper. This basic concept, from which the other 'secondary' meanings emanate, has an impact on the meaning of *dudgeon*. W. defines it thus: "an irate fit of indignation". How has this definition been arrived at? There are no related forms which can give clues as to the interpretation of the noun (like *anger*: *angry*; *fury*; *furious*, *infuriate* etc.). The concept 'action' has obviously been projected on the noun itself so that it denotes 'violent reaction'.[4] But why is *high* obligatory? *Dudgeon* should be able to stand on its own feet, as it suggests 'violence'. The explanation must be sought in the principle of semantic redundancy. The noun containing the element 'violence', which has as it were been pushed into it by *walk out*, attracts *high* to itself.[5] *High* is an appropriate adjective to be attracted, as there is a class of nouns related with *dudgeon* figuring in its range: *indignation*, *fury*, *temper*. Bolinger (1972) says about *high*: "its literal meaning sorts well with what is florid, visible, and impetuous".

5 In the collocations with adjectives as nodes discussed below, there is a tendency towards monopolization, i.e. one single lexical item occupies a strikingly prominent place in the range of its adjective. As both items figure largely in each other's ranges, there is bidirectionality in the semantic overflow.

5.1 As a basis for the discussion let us consider the collocational pattern of *brazen*: (the figures stand for the number of responses)

hussy 44 woman 2 look 3 face impudence nerve image sky

Let us assume that a person is faced with the task of finding out what *brazen hussy* means by analysing the range. He knows the meaning of all collocates except *hussy* and the basic meaning of *brazen*, 'made of brass'. He makes a preliminary definition of *hussy*: 'a woman who is impudent and shows nerve'. In order to arrive at a more precise definition he tries to examine the crucial words, *impudence* and *nerve*, by means of an extensive dictionary (W.). He finds a common denominator: 'shamelessness', which is also registered as an obsolete sense of *impudence*. Furthermore, he finds a link between this basic sense and 'hardness' in that *hardihood* is said to be a synonym of *nerve*. *Brazen* is registered as a synonym of *hardy*. He thus concludes that the figurative use of *brazen* has basically the sense 'shameless', to which the component 'hard' is added.[6] He is puzzled by the illogicality of the definition which this leads up to: *hussy* 'shameless woman'; *brazen* 'shameless', with a suggestion of 'hardness'. Consequently, *brazen hussy* is tantamount to 'shameless, shameless woman'.[7] In his confusion he looks up the words to see what mistake he has made. But the dictionary offers no way out. *Brazen* is said to be a synonym of *shameless* ("brazen hussy" serves as an illustrative example), and *hussy* is defined: "a lewd or brazen woman".

This collocation is an excellent illustration of the principle of semantic redundancy. This redundancy is the reason why *hussy* tends to monopolize *brazen* and *brazen* tends to monopolize *hussy* (this was tested with 10 informants: *brazen* 7 cheeky little mean), or the monopolizing tendency might be said to have caused this redundancy. There is a bidirectional semantic overflow in this frozen collocation. Thereby *brazen* has come to include all semantic markers or components in *hussy* (arrogance, bitchiness, sexual immorality etc.), and *brazen* serves to intensify the *'hussiness' of a woman. 'You are a brazen hussy' means 'You are utterly shameless, bitchy, arrogant etc.'. Used as a predicate, *brazen* has, the informants told me, lost its derogatory sense almost entirely because of the changed attitudes to women as regards dress, sexual behaviour etc.. "There is a difference between *calling* someone 'brazen' and *describing* someone as 'brazen'", one informant said.

The close semantic link between the two items causes syntactic frozenness. *'The hussy is brazen' is ruled out because a 'hussy'

cannot be *described* as 'brazen'. It is as nonsensical as *'the woman is female'. Nor is it possible to modify the adjective; *'a very brazen hussy' would be tantamount to *'a very shameless, shameless woman'. The informants said that *very* is impossible because it would separate *brazen* from *hussy*. Obviously *'a very brazen hussy' is as impossible as *'a very blackboard'.

The metaphorical process which has given rise to the collocation *brazen hussy* is fairly transparent. The superordinate semantic feature of the range of *brazen* is 'hardness'. The association between 'hardness' and 'shamelessness' is a natural one, evinced in the synonymousness of *nerve* with *hardihood* and the figurative sense of *brass*. Consequently, there is a high degree of *semantic cohesion* in the range of *brazen*.

5.1.1 A semantic cohesion of a different nature has been found in the ranges of many other adjectives, for example, *fractious*:

child 27 baby 6 class infant mare pupil underling remark temper

The superordinate semantic feature is obviously 'difficulty of managing' in that the nouns signify 'person or animal under the authority of somebody'. *Remark* and *temper* are "islands" in the range, but it is obvious that they should be interpreted 'typical of a child etc.'. The relation between *fractious* and its head is 'instantial', i.e. *fractious child* is an instance of 'child in general', whereas the relation between *brazen* and *hussy* is 'essential', i.e. 'brazen' represents the essence of the imaginary English word *'hussiness'. A child is either 'fractious' or 'not fractious', whereas a *hussy* is necessarily 'brazen'.

5.1.2 We are faced with a puzzling problem when we consider the range of *rank*:

outsider 16 / growth 3 weeds 2 undergrowth / smell 5 odour / bad manners dishonesty 2 disobedience insolence insult stupidity[8]

The range consists of four groups with no obvious semantic link between them. Furthermore, there is no semantic cohesion in the group 'abstract nouns denoting negative qualities' (cf. the close link between *impudence* and *nerve* in the range of *brazen*). How, then, is the main collocate to be accounted for? Trying to base a definition on the nature of the range, one finds oneself at a loss. Does *outsider* denote 'a person who is disobedient, bad-mannered, insolent', or what? W. says: "a person not recognized or accepted as a member of a group, category, or organization". There are two possible explanations of this specific attraction:

(1) Some dictionaries record for *rank* the sense 'bad-mannered', ill-bred, vulgar person'. Following this, *bad manners* might be the clue. *Rank outsider* may have a strongly derogatory sense 'a person with rank bad manners'. This interpretation is to a certain extent supported by H.: "*He is an outsider = Il n'est pas de notre monde*"; "*He is a rank outsider = C'est un intrus dans notre monde*". A 'person who makes an intrusion' suggests 'bad manners', 'vulgarity'. But a person can be an 'outsider' for a number of reasons: 'belonging to another political party' (*political outsider* is registered by W.), 'being upper class/lower class etc.', or simply 'not being an expert', as is shown by W.: "that is ... the opinion of a rank outsider".

(2) One informant, in a discussion of the difference between *luxuriant growth* and *rank growth*, asserted that *rank growth* is the opposite of *luxuriant growth*, "nothing grows". Obviously, this is a 'leap' from 'strong' to 'weak'. *Rank* is erroneously transferred from its common collocation in *rank weeds* to the *rank growth* (poor growth) of the garden plants. These do not stand a chance, they are *rank outsiders*.

According to (1), *rank outsider* arose from a restricted use of the noun, with a secondary sense which is strongly pejorative. This sense is not retained, as the informants reject it. Consequently, *bad manners* in the modern use is a false clue. As we have seen, *brazen* in the collocation *brazen hussy*, whose constituents are welded together by semantic overflow, keeps its expressive vitality intact because of its link with *hussy* but has lost its condemnatory force as a predicate ('the woman is brazen' was said to be 'unnatural' by the informants). Moreover, *rank* seems to demand a negative noun as head in the group of nouns designating qualities (*rank bad manners*, but hardly **rank manners*; equally unlikely is **rank person*). *Brazen*, on the other hand, is semantically strong enough to convey the derogatory sense independently (*face*, *look*, *woman*). In spite of this the adjective feels most comfortable in the company of *hussy*, which imbues it with expressive strength. Such a dependence would seem to be even more necessary for *rank*. The clue to the interpretation of *rank outsider* is to be found in (2), I think, but the analysis must be reconsidered. The superordinate semantic feature in the range of *rank* is 'repulsion'. There is a polarity involved between what repels and what is repelled.[9] The collocates *weeds*, *undergrowth*, and *smell* suggest that 'choking/dense (cf. *crass stupidity*) abundance' causes this repulsion. The 'leap' represented by the informant's interpretation of *rank growth* seems to have caused the collocation *rank soil* (ALD), *rank* having been transferred from *weeds* to *soil*. In both examples there is a polarity between the 'odious abundance' of the *rank weeds* and the sterility of the *soil* (the 'leap' in the informant's reaction was probably a transfer of *rank* from *growth* to *soil*). Thus the weeds are *rank outsiders*. The basic negative element in *rank* must be preserved. The idea of 'abundance' and 'repulsion' has vanished altogether in the modern use of this frozen collocation. It may remain in some situations in an extremely vague sense: an 'outsider' is likely to cause

embarrassment in the group to which he does not belong. The puzzlement in the preliminary analysis was due to what seemed to be a reversal of direction in the superordinate feature 'repulsion'. *Rank smell*, *rank weeds*, *rank dishonesty* repel, but a *rank outsider* is rather *the object* of repulsion. There is no semantic overflow in this collocation, because it is semantically fossilized, i.e. the metaphorical process by which it has arisen has lost its vitality. As a consequence, *rank* does not have much meaning, if any.[10] *Rank outsider* is rather like *arrant nonsense* in that the range does not offer any obvious clue to the problem of how the specific attraction can be explained. One informant said: "*Nonsense* has stronger emphasis than *arrant nonsense*." Another indication of the difference between *brazen hussy* and *rank outsider* is the reasons informants gave for *very* being impossible. With *brazen*, *very* would imply "pulling away" the adjective from its noun, with *rank*, *very* would be nonsensical because *rank* means 'complete'.

5.1.3 Another case of puzzling isolation of the main collocate is observed in the range of *dismal*:

failure 13 outlook 6 future prospect day 4 weather 3

(The low number of responses is due to the fact that *dismal* was tested only in the interviews).

This is a "fashionable collocation" (informants). Bolinger (1972) lists it in a group of clichés, but none of the dictionaries which I have consulted records it. The degree of cohesion is not high enough to rule out intensification (*very* was accepted by most informants). Only with *failure* is *dismal* 'essential'; with *outlook* etc. it is 'instantial', i.e. an *outlook* is either *dismal* or *rosy*. *Failure* is essentially 'a dismal affair'.

Thus, contrary to what is the case with *brazen* and *rank*, *dismal* is used as an intensifier only with its main collocate. It might have received this function through a phonetic misinterpretation of *abysmal*, whose second collocate is *failure* (6x) and whose function is to express degree (main collocate: *ignorance*). Being more frequent, *dismal* is likely to oust *abysmal* altogether from the range of *failure*. Moreover, the link between *dismal* and *failure* is apt to become as strong as the link between *brazen* and *hussy* because there is a degree of semantic redundancy in the collocation. *Dismal* will lose its syntactic freedom completely without a similar loss of semantic expressiveness. *Rank outsider*, as we have seen, is entirely different.

6 Full-fledged adjectives originating in present participles of verbs are of the type:

The mishap confused John --- a confusing mishap

Other examples are *interesting*, *frightening* etc.. They have syntactic freedom: a very interesting book/the book is interesting/an interesting old book etc.. They are not likely to attract a specific noun. *Withering look/glance* might be said to be an exception, but this is a 'transform' of 'Somebody withers John with a look' rather than of 'A look withers John'. Adjectives originating in intransitive verbs are entirely different. They are syntactically frozen and they tend to be monopolized by a specific noun. Examples are *blithering idiot* , and *raving lunatic*. Such collocations are marked by semantic redundancy in that the activity denoted by the verb is *essentially characteristic* of the noun which monopolizes the adjective.

6.1 Let us consider the collocation *raving lunatic*. It might be paraphrased: 'a person showing wild eccentricity to the point of raving', or rather, 'a person who is so wildly eccentric that he resembles a lunatic'. When the speaker appends *raving* to *lunatic*, the process is the following: a semantic feature of the figurative sense of the noun is extracted, then scaled up, high-lighted as it were (by the lexemic realization of a feature inherent in the 'basic' sense of the noun: an insane person usually talks incoherently) for a melodramatic intensifying effect. The range of *raving*:

lunatic 38 maniac 3 madman lunacy madness beauty success[11] / mad 10

The adjective *mad* tends to overshadow the collocation with *lunatic*. In spite of this the noun *madman* has a very modest place in the range. The concept expressed by *lunatic* is obviously more closely related with the idea 'talk incoherently' (*raving*) than the concept expressed by *madman*. This is confirmed by W.: *lunatic*: "wildly eccentric"; *madman*: "rash or foolish"; *maniac*: "characterized by an inordinate or ungovernable enthusiasm". Evidently *lunatic* is the 'strongest' word. This poses a 'chicken or the egg' question: was *lunatic a priori* 'strong' enough to attract *raving*, or, has *raving* imbued *lunatic* with this 'strength' by semantic overflow through a gradual increase of the degree of attraction, finally reaching the stage of near-monopolization?

6.2 *Blithering idiot* rather resembles *raving lunatic*, but there are two important differences: (1) *idiot* is the only collocate of *blithering*, which probably owes its existence to sound-symbolism; (2) *blithering* does not rank so high in the range of *idiot* (the main collocate is *congenital* according to the informants) as *raving* does in the range of *lunatic*.

7 The phenomenon of monopolization or tendency towards monopolization in frozen collocations of the type discussed has a bearing on matters of principal interest in linguistics, such as the relation between syntax (or lexotactics) and semantics

(or semotactics), the status of the concept 'word', synonymity, psychological processes involved in lexical selection etc..

7.1 The collocations discussed represent various degrees of *lexicalization*, i.e., a tendency to form a single lexeme or a lexical unit, reflected in their syntactic frozenness:

(1) *High dudgeon* must be regarded as a unitary lexeme because the adjective is obligatory for the manifestation of the noun in the same sense as *warm* is obligatory for the manifestation of *luke* in the single 'word' *lukewarm*. *Good* is just as obligatory for *riddance* as *high* is for *dudgeon* but differs in two ways: *riddance* is not unique to the same extent as *dudgeon* because of the lexemes *get rid of* and *be rid of*, and *good riddance* does not show the same degree of syntactic petrification as *high dudgeon*; some informants said *jolly good* was possible in the environment *and jolly good riddance too*. This is probably due to the fact that *good* is, as we have seen, the carrier of the basic sense (*riddance* is semantically redundant). *Good mixer* quite normally takes *very* and therefore does not evince the same unit character as *good riddance* and *high dudgeon*.

(2) *-ing*-adjectives based on intransitive verbs have a strong monopolization tendency. They tend to form lexical units with their heads. Examples: *crashing bore*, *crying shame*. *Raving lunatic* is particularly interesting because *raving* is restricted to one specific sense of *lunatic*. This is a characteristic feature of many 'quasi-adverbs' (OED's term), evinced by the collocations *raving mad* ('very crazy') and *hopping mad* ('very angry'). These adverbs are syntactically different from ordinary adverbs in that they lack *-ly*. Moreover, the adjectives when 'modified' by these quasi-adverbs, do not take nouns: *'a raving mad person'. The degree of lexicalization in 'quasi-adverb'/adjective-combinations is obviously higher than in *-ing*-adjective/noun-combinations. Even such an adjective as *blithering* obviously allows other nouns than *idiot*: *fool* was accepted "as an extension" by the informants. But *raving* is completely monopolized by *mad*, **raving crazy* is impossible.[12]

A refined, or a more delicate, lexicalization is evident in some collocations of this type in that the topics are restricted to specific types of nouns. Examples are *blazing hot* (sun, air), *boiling hot* (usually tea and coffee) and *piping hot* (tea, food).

Collocations like *brazen hussy* show the same type of lexicalization as *raving lunatic* in that the adjectives rank high in the ranges of the nouns without being obligatory.

7.2 The principle of semantic redundancy in collocations with adjectives as nodes implies, generally speaking, a fading of the literal sense of the adjectives. Thus they express degree by repeating the most essential feature of their nouns, which are gradable: 'rather a hussy/failure' etc.. But by the semantic

overflow between the participants in firmly established collocations the adjectives are much more expressive than a relatively grammaticised intensifier. Thus the intensification of *brazen* and *dismal* with *hussy* and *failure* is much stronger than *great* or *damned*, which are completely grammaticised, having no preference in the selection of degree nouns.

The collocations with nouns as nodes are different in that the nouns are modified obligatorily by adjectives in whose ranges they are not very prominent (*dudgeon* might be said to be an exception, because it is part of a class of nouns which take *high* as their modifier). *Mixer* and *dudgeon* are scalable; 'sociability' and 'anger' can characterize a person to a higher or lower degree. By the obligatoriness of the adjectives, the nouns, when given an independent definition, contain the feature 'high degree of'. If asked what 'mixer, used about a person' (we assume that the informant is told that 'mixer' with the constructive definition, denoting a profession, is ruled out) means, an informant would probably say something like 'a person who is perfectly at ease with other people'. The adjective as it were intensifies something which is already intensified. Thus we face a paradox: *the adjective is completely redundant and obligatory* (the paraphrases might be said to be 'high, high degree of sociability/anger/indignation'). What about *riddance*? The concept 'separation' is certainly not gradable. But the combination *good riddance* denotes a gradable concept, 'relief', which tends to be 'pushed into the noun', as we have seen from W.'s definition of *riddance*. The reason for this misleading definition is that the hearer decodes messages as *content* and *form*. Content is hazy, form is sharp. Utterances are segmented according to the 'word' concept, and the words are given 'content shapes' with a sharpness which is unjustified, owing to the phenomenon of semantic overflow. An indication of this is the fact that some informants said that 'I am well rid of him' means 'he is completely out of the way'.

But semantic redundancy is not restricted to degree nouns. *Glance*, for example, is the main collocate of *quick*, *fleeting*, and *cursory*, which all express concepts which are inherent in the noun. *Clenched* elicited only *fist*, which denotes 'a clenched hand'. Other examples of similar elicitations are *wealthy landowner/tycoon*, *husky he-man*, *prissy righteousness*, and *righteous indignation* (main collocate).

7.3 Is there any evidence for this attraction between words expressing similar concepts and semantic overflow? Let us consider some facts that might back up the arguments:

(1) The morph *luke* in *lukewarm* originally meant 'warm'. Like attracts like.

(2) *Hectic* used to be monopolized by *fever*, meaning 'normal'. It has changed its meaning to 'marked by feverish activity'; its main collocate is *day*. The monopolizer leaves a semantic imprint on its 'satellite'.

(3) Coinages by widely-read authors are apt to become firmly established collocations, if there is an element of semantic redundancy. *Artful* elicited only *dodger*. Not all informants were aware of the origin of the collocation, Dickens' *Oliver Twist*.

8 Applications

The significance of lexical environment for the interpretation of the content of 'words' would seem to justify the inclusion in dictionaries of collocational descriptions of lexical entries, based on various types of statistical material. Such descriptions would be a valuable complement of dictionary definitions, which to a great extent are based on the habitual contexts of the lexical entry. Thus a definition and a description of the collocational pattern elucidate each other. Needless to say, descriptions of synonyms in collocational terms would obviously have a higher degree of precision than those found in the standard dictionaries of synonyms with their random illustrative examples, often from various centuries. Descriptions of this type should be included in what Makkai (1974) calls "TRUE" dictionaries ("Thesaurus rendered updated ecologically").

8.1 Let me make a very tentative analysis of *task* on these lines. *Task* is the main collocate of three adjectives in my material. These adjectives may be taken to represent various semantic features, typical of *task* (that is not to say that the features are *inherent* in *task*):

(1) 'Lowliness'

menial: task 44 job 4 chore 2 duty employment labour position work

(2) 'Difficulty' + 'irksomeness' (*task* is listed with *irksome* in W.)

onerous: *task* 18 burden 4 responsibility 4 terms (only tested in the interviews)

(3) 'Tedium'

(a) *tedious:* *task* 9 job 8 work 5
(b) *humdrum:* task 4 job work (main collocate: *existence* 17)
(c) *monotonous:* job 4 work task (main collocate: *voice*)

The central feature is (1), which implies 'piece of work imposed on a person', reflected in 'a set task', 'set somebody a task'. What one is told to do is primarily assessed according to its complexity. *Task*, like *question*, revolves around the axis 'difficult-easy'. But unlike *question*, *task* is attracted by the 'difficult'-end of the

axis. "Since the word usually connotes difficulty or strain, it is often qualified by *hard*, *difficult* etc., but seldom in precise use by their opposites" (WS). This is further backed up by (3), which evinces a link between *task* and *difficulty*. There is competition between *task*, *job*, and *work* in (3) in spite of the negative overtones of *task* and the neutral import of *job* and *work*. The reason for this is the element 'permanence' inherent in *job* and *work*. *Job* has also a 'temporary' sense, but with the three adjectives in (3), this noun is likely to have its 'permanence' value. This is backed by the predominance of *job* over *task* in *monotonous*, which stresses 'continuance', and the predominance of *task* over *job* with *humdrum*, which does not have the 'continuance' element to the same degree as *monotonous*. This is revealed by the collocation *humdrum monotony* (one occurrence in my informant test). *Monotony* contains two features, 'non-excitement' and 'continuance'. *Humdrum* extracts and emphasizes 'non-excitement' (semantic redundancy). The range of *task* in BC lends support to my arguments:

difficult 4 complex critical delicate gargantuan Herculean monumental trivial easy simple daily

Task is evidently attracted by the 'difficult'-end of the 'difficult-easy' axis. *Gargantuan*, *Herculean*, and *monumental* represent a tendency towards 'too difficult', *trivial* shows its affinity with *chore*. The concepts 'difficulty', 'irksomeness', 'strain' etc., can be expressed by *task* independently: "He made a *task* of writing a letter" (WS). This is further proof of the flexibility of the content of 'words'.

8.2 By comparing the ranges of *imminent* and *impending* I will try to illustrate how the semantic relation between synonyms can be elucidated in collocational terms.

imminent: *disaster* 19 danger 18 storm 2 arrival event

impending: *doom* 17 disaster 7 calamity charges trial storm (only tested in the interviews)

There is a higher degree of monopolization in the range of *impending* than in the range of *imminent*. *Doom* is highly significant for *impending*. The feature 'inevitability' in *doom*, also present in *charges* and *trial*, is crucial. The note of threat, present in both adjectives, is much stronger in *impending*. This interpretation is backed up by the neutral nouns in the range of *imminent* (*arrival*, *event*). (The difference between the adjectives was also corroborated by the informants.) The high degree of the 'threat' notion in *impending* has an interesting psychological implication. Contrary to what some dictionaries state, for example, Meldau-Whitling, and to what 'his life was in imminent danger' suggests, many informants said that *impending* was "nearer" than imminent. This is obviously

related with the feature 'inevitability'.

9 Concluding remarks

The discussion in this paper of the principle of semantic redundancy, manifested in many frozen collocations, is an implicit criticism of the *componential analysis* method in Katz-Fodor. I have tried to show that this principle is often combined with a semantic overflow between the lexical items, which are welded together in such types of collocation. The flexibility of the content of lexical items, which is a corollary of my arguments, is incompatible with the 'hardness' which characterizes lexical items in Katz-Fodor's approach. Bolinger (1965) has criticized this approach convincingly, and a quotation from his article, where he comments on Katz-Fodor's view of lexical units, aptly sums up my basic arguments:

> "They (lexical units) are brittle pieces of crystalline structure that may be picked up but not absorbed: 'The dictionary is something that the speaker learns item by item, more or less by rote' (183 footnote). The mental picture to which this gives rise is that of a transaction in which one comes wholly into possession of a piece of property all at once, like buying a parcel of real estate, rather than the gradual EMERGENCE of meaning, for the speaker, through a long process of decontextualisation in which a word is only dimly grasped at first, and slowly, as it gains in context, cancels its over-extensions one by one."

FOOTNOTES

1. "*Their departure was a happy riddance*", recorded by H., was flatly rejected by the informants. The main collocate of *arrant* is *nonsense*.

2. Note that **I am sorry to get rid of him* is equally nonsensical.

3. When used as a degree word, *inspector* does not drop this component, because it is exactly this component which is gradable in *he is a regular inspector* (he behaves as if he were an inspector, examining everything very carefully). Only the adjective *regular* can make such nouns gradable (lawyer, bachelor etc.).

4. *Umbrage*, which is listed as a synonym of *dudgeon* by W., suggests an entirely different reaction. Occurring exclusively after the verb *take*, which has obviously ousted *give* (*give* and *take* used to have the same status to judge by OED), the basic feature is 'passivity'. According to W., *umbrage* suggests "blended hurt pride, jealousy, suspicion of another's motives, and ill will". Thus *umbrage* suggests a weak ego. *Dudgeon* implies openness, whereas *umbrage* implies concealment. The difference between the two reactions is evinced by the following tentative definitions:
Dudgeon: irate fit of indignation, which is healthily acted out and quickly disposed of.
Umbrage: concealed ill will, which rankles in the mind.

5. W.'s illustrative examples do not contain 'walk out' but all of them have verbs denoting the same basic idea, like "stalk off", "refuse to sit with" etc..

6. Note that there is an overlap between the concepts expressed by 'shameless' and 'hard': 'showing no shame' implies 'having a hard attitude'.

7. This phrase has an entirely different stylistic value (according to the informants, it has a literary, stilted note) from *brazen hussy*, although it conveys essentially the same idea.

8. The informants were asked to give their responses without reflection and to leave an adjective 'unanswered' if a noun did not spring to mind immediately. Thus the total number of responses varies for different adjectives. This gives a clue as to the frequency of the adjectives. *Rank* , having only 43 responses, is obviously rather infrequent.

9. This polarity is curiously reflected in the range in that some of the abstract nouns have distinct antonyms: *bad* manners, *dishonesty* and *disobedience*: W. has *disloyalty*.

10. The absence of denunciatory overtones is evident in: "most of the actors were not big names, but rank beginners" (W.). *Beginner* was accepted by the informants as an "extension".

11. In the collocations with *beauty* and *success* no redundancy is involved, of course. The feature is transferred to the speaker: 'so enthusiastic that he raves'.

12. As with *high* and *highly*, which are restricted to polosyllables (Bolinger 1972), a phonological restriction is involved with these adverbs: only monosyllables are permitted. This restriction does not apply to other 'quasi-adverbs', like *stark*, which is completely monopolized by *naked*. Compare *stark naked* with *stark staring mad*. There is an exact parallel in Swedish: *spritt naken* 'stark naked'; *spritt språngande galen* 'stark staring mad'.

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A TAGMEMIC MODEL OF ITALIAN VERB MORPHOLOGY

Marcel Danesi

University of Toronto

1. Practically no work of a tagmemic nature has been done in the field of Italian linguistics. However, as Brend (1968) has demonstrated, this model seems to be particularly suitable for describing the highly-inflecting Romance languages. The purpose of this paper is to apply the theory and methodology of tagmemic analysis to Italian verbs, and specifically to that subclass known as the regular finites. At the same time, it will also be our purpose to investigate the nature of the tagmemic model for verb inflection and compare it to alternative morphological descriptions of the regular finites.

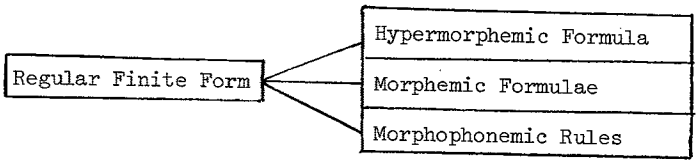
For the identification of morphemes (or allomorphic variants), structural linguistics provides a fairly reliable method akin to the well-known common factor analysis of elementary algebra: e.g., $ax + bx + cx = x(a + b + c)$. The $\underline{x}$ is a common factor in this algebraic expression because it occurs in each term. In linguistic studies, this type of analysis is known as segmentation and is carried out by the method of form comparison. Once the constant meaningful units are isolated, the analyst is then confronted with the problem of grouping the units (morphemes) and deriving rules of combination (see Nida 1949:6-61; Elson and Pickett 1969:7-52).

To solve this problem, tagmemics has developed the concept of the tagmeme, which is the unit of grammatical arrangement that defines a correlation of grammatical function to form (see Cook 1969: 15-17). At the word level, typical constructions consist of tagmemes which are manifested by stems and affixes. Word constructions then form a distinct level in a grammatical hierarchy, i.e., they fill slots at the phrase level (see Elson and Pickett 1969:76). This model allows the analyst to investigate certain relationships which he may then fit into a larger and even eclectic theoretical framework, as Belasco (1961) did for Old French by combining transformational and tagmemic techniques.

2. In our analysis of the regular finites, we must first of all develop a working hypothesis on the nature of verbal regularity. Regularity in verbal inflection consists of some recurrent pattern in the verbal paradigms. From a descriptive standpoint, this general pattern may be expressed by some formal apparatus or algorithm which allows one to derive all the paradigms. In other words, for the Italian regular finites (if they are indeed regular) there exists a concise program of instructions which, if read by a computer, will generate all the paradigms.

As in mathematics, one of the most convenient ways of relating or conveying information is to give an underlying formula for calculating $\underline{n}$ quantities of a structure. For example, Euler's formula $e^{\underline{ix}} = \cos \underline{x} + \underline{i} \sin \underline{x}$ makes possible the derivation of all the trigonometric tables (see Smith 1953:613). The tagmemic model that we

propose will provide such a general derivational formula at a higly abstract level of representation which we shall call hypermorphemic (as in stratificational studies). Paradigmatic slot-filling for each specific tense (imperfect, future, conditional, past absolute, present) then occurs at a lower morphemic level. Finally, phonetic variations in the items filling the slots occur at a lower morphophonemic level: in our model, this level handles all cases of predictable variation in the phonetic shape of the fillers. This model may be represented by the following schema:



It should be noted that the word (regular finite form) manifests a simultaneous three-level layering: it fits some general pattern expressed by a canonical form at the hypermorphemic level; it has specific slot-fillers for its paradigms at the morphemic level; its fillers undergo regular phonetic processes at the morphophonemic level. For a similar conception of word morphology, see Matthews (1972:55).

Italian has three subcategories of regular finites which we shall symbolize as rf_1, rf_2, rf_3. Traditionally, these subcategories are referred to as the first, second, and third conjugations; class membership is usually decided on the basis of infinitive suffixes with which each subcategory stem occurs. However, since these suffixes also may occur with non-regular stems (which show complex and heterogeneous changes) we discard this criterion. All regular finite stems are phonetically invariable. Therefore, in our model, class membership is based simply on lexical classification: i.e., when phonetically constant stems enter into paradigms which fit the general pattern expressed by the hypermorphemic formula, then they are listed as regular finites and are subcategorized in the lexicon on the basis of the various tagmemic constructions found at the morphemic level. As in most classifications of this type, a certain amount of circularity emerges. However, as Gödel (1931) has proved, this is true of any set of propositions or axioms.

2.1 We propose the following hypermorphemic formula as the general statement which reveals the underlying structure of all regular finites in all moods and tenses:

rf = + vnuc:rfs + svnuc:sv + tm:tm + pn:pn

This formula states that any regular finite verb in any tense consists of the following obligatory tagmemes: (1) a verb nuclear slot filled by any regular finite verbal stem; (2) a stem vowel nuclear slot filled by any stem vowel; (3) a tense-mood slot filled by a tense-mood marker; (4) a person-number slot filled by a person-number marker. As Elson and Pickett (1969:77-78) indicate, in word-level tagmemes it is often difficult to separate function (indicated by the slot name) and the class meaning of the fillers, for classes of affixes are apt to have uniform class meaning and function: hence, the + tm:tm and + pn:pn

tagmemes of our formula.

2.2 The first tense we shall examine is the imperfect (indicative and subjunctive). The morphemic formulae for this tense show overt markers filling the three affix slots of the general hypermorphemic formula. As examples, we shall use the regular finite stems /kant-/ 'sing', /tem-/ 'fear', and /dorm-/ 'sleep' which typically fill the + vnuc slot. The paradigms for the imperfect may be charted as follows:

Imperfect Indicative

rf_1	rf_2	rf_3	
/kantávo/	/temévo/	/dormívo/	'I was singing, fearing, sleeping'
/kantávi/	/temévi/	/dormívi/	'you (sing.) were singing', etc.
/kantáva/	/teméva/	/dormíva/	'he, she, it was singing', etc.
/kantavámo/	/temevámo/	/dormivámo/	'we were singing', etc.
/kantaváte/	/temeváte/	/dormiváte/	'you (pl.) were singing', etc.
/kantávano/	/temévano/	/dormívano/	'they were singing', etc.

Imperfect Subjunctive

/kantás:i/	/temés:i/	/dormís:i/	'that I used to sing, fear, sleep'
/kantás:i/	/temés:i/	/dormís:i/	etc.
/kantás:e/	/temés:e/	/dormís:e/	
/kantás:imo/	/temés:imo/	/dormís:imo/	
/kantáste/	/teméste/	/dormíste/	
/kantás:ero/	/temés:ero/	/dormís:ero/	

The segmentation analysis of these and similar paradigms yields the following morphological information: (1) /-v-/ marks imperfect indicative and /-s:-/ imperfect subjunctive; (2) for rf_1, rf_2, and rf_3 the fillers of the + svnuc slot for both moods are /-a-/, /-e-/, /-i-/ respectively; (3) the + pn slot is filled by the ordered set pn_1 = (/-o/, /-i/, /-a/, /-amo/, /-ate/, /-ano/) in the indicative and pn_2 = (/-i/, /-i/, /-e/, /-imo/, /-te/, /-ero/) in the subjunctive, where the elements of these sets are the personal endings (first person singular, second person singular, etc.).

Two phonetic facts also emerge: (1) the tonic accent is normally on the stem vowel except for the first and second person plural of the imperfect indicative for all subcategories; (2) the /-s:-/ marker becomes /-s-/ before the personal ending /-te/. These facts are formalized at the morphophonemic level by process rules (as in transformational studies):

mph_1: for any imp ind rf, + vnuc + svnúc + tm + pn $\longrightarrow$ + vnuc + svnuc + tm + pń, when pn = (/-amo/, /-ate/)

mph_2: + tm:/s:/ $\longrightarrow$ + tm:/s/ / __ +pn:/te/

In our morphemic formulae, we shall use the symbol $/s/_{mph2}$ to indicate that this filler is the output of rule mph_2. In general, any morphophonemically conditioned filler x will be symbolized as $/x/_{mph_n}$. The morphemic formulae for the imperfect are:

imp ind rf_1 = + vnuc:rf_1s + svnuc:/a/ + tm:/v/ + pn:pn_1

imp ind rf_2 = + vnuc:rf_2s + svnuc:/e/ + tm:/v/ + pn:pn_1

imp ind rf_3 = + vnuc:rf_3s + svnuc:/i/ + tm:/v/ + pn:pn_1

imp subj rf_1 = + vnuc:rf_1s + svnuc:/a/ + tm:/s:/-/s/$_{mph2}$ + pn:pn_2

imp subj rf_2 = + vnuc:rf_2s + svnuc:/e/ + tm:/s:/-/s/$_{mph2}$ + pn:pn_2

imp subj rf_3 = + vnuc:rf_3s + svnuc:/i/ + tm:/s:/-/s/$_{mph2}$ + pn:pn_2

2.3 The paradigms for the future and conditional are:

Future

rf_1	rf_2	rf_3	
/kanteró/	/temeró/	/dormiró/	'I shall sing, fear, sleep'
/kanteráj/	/temeráj/	/dormiráj/	etc.
/kanterá/	/temerá/	/dormirá/	
/kanterémo/	/temerémo/	/dormirémo/	
/kanteréte/	/temeréte/	/dormiréte/	
/kanterán:o/	/temerán:o/	/dormirán:o/	

Conditional

/kanteréj/	/temeréj/	/dormiréj/	'I should sing, fear, sleep'
/kanteresti/	/temeresti/	/dormiresti/	etc.
/kanteréb:e/	/temeréb:e/	/dormiréb:e/	
/kanterém:o/	/temerém:o/	/dormirém:o/	
/kanteréste/	/temeréste/	/dormiréste/	
/kanteréb:ero/	/temeréb:ero/	/dormiréb:ero/	

In these paradigms, a segmental analysis shows that: (1) /-r-/ marks future and /-re-/ conditional; (2) the + pn slot is filled by pn_3 = (/-o/, /-aj/, /-a/, /-emo/, /-ete/, /-an:o/) in the future and by pn_4 = (/-j/, /-sti/, /-b:e/, /-m:o/, /-ste/, /- b:ero/) in the conditional; (3) as for the imperfect, /-e-/ and /-i-/ fill the + svnuc slot for rf_2 and rf_3 respectively. The problem that emerges is the appearance of /-e-/ instead of /-a-/ for the stem vowel in rf_1 paradigms. The solution is simply a morphophonemic rule:

$$mph_3\text{: + svnuc:/a/} \longrightarrow \text{+ svnuc:/e/ / \_\_ + tm:/r/} - /re/$$

This palatalizing effect on /-a-/ by /-r-/ has also been observed by De Taeye-Henen (1969:51). There are no accent shifts in these paradigms: in the future the tonic accent falls on the + pn slot; in the conditional it falls on the + tm slot.

The relevant morphemic formulae are:

fut rf_1 = + vnuc:rf_1s + svnuc:/e/$_{mph3}$ + tm:/r/ + pn:pn_3

fut rf_2 = + vnuc:rf_2s + svnuc:/e/ + tm:/r/ + pn:pn_3

fut rf_3 = + vnuc:rf_3s + svnuc:/i/ + tm:/r/ + pn:pn_3

cond rf_1 = + vnuc:rf_1s + svnuc:/e/$_{mph_3}$ + tm:/re/ + pn:pn_4

cond rf_2 = + vnuc:rf_2s + svnuc:/e/ + tm:/re/ + pn:pn_4

cond rf_3 = + vnuc:rf_3s + svnuc:/i/ + tm:/re/ + pn:pn_4

2.4 The paradigms for the past absolute may be charted as follows:

Past Absolute

rf_1	rf_2	rf_3	
/kantái̯/	/temé̯i/ or /temét:i/	/dormí̯i/	'I sang, feared, slept'
/kantásti/	/temésti/	/dormísti/	etc.
/kantó/	/temé/ or /temét:e/	/dormí/	
/kantám:o/	/temém:o/	/dormím:o/	
/kantáste/	/teméste/	/dormíste/	
/kantárono/	/temérono/ or /temét:ero/	/dormírono/	

The segmentation analysis of these forms yields the following facts: (1) /-a-/, /-e-/, and /-i-/ fill the + svnuc slot except in one case (/kantó/); (2) the + tm slot has no overt marker, and we thus postulate a Ø filler for this slot; (3) the + pn slot is filled by pn_5 = (/-i̯/, /-sti/, -Ø, /-m:o/, /-ste/, /-rono/); (4) for rf_2 verbs, /-t:-/ alternatively fills the + tm slot only in the first person singular and the third person singular and plural; the appropriate person-number markers when /-t:-/ is employed are: /-i/, /-e/, /-ero/.

Two problems emerge from this analysis: (1) the use of Ø; (2) the form /kantó/. The use of Ø as a filler for + tm, rather than an optional tagmeme, is required by the process of generalization and the criterion of regularity as expressed in the general formula (at the hypermorphemic level). Otherwise, an ad hoc description would have to be given for the past absolute. The same applies to the use of Ø as an element in pn_5 (third person singular).

The Ø device is obviously a creation of the linguist, which may or may not have psychological reality, in the same way that the numeral 0 is a mathematical fiction. As in mathematics, the Ø in our formula marks a positional relationship; i.e., it is an implicit sign understood through its relationship with other units of the given paradigmatic class (see also Haas 1957; Khlebnikova 1973:52-55; Schifko 1973). In most studies, Ø is justified if it does not have emic status. As Di Pietro (1963:57) points out: 'It is theoretically feasible to postulate zero allomorphs but not zero morphemes'. Our Ø filler is, in effect, one of the allomorphs of + tm; others, as already indicated, are /-v-/, /-s:-/, /-r-/, and /-re-/. In tagmemic terms, + tm:Ø is an allotagma of a tagmeme (+ tm:tm) which is overtly marked in other cases. Even for the past absolute, we find an overt marker /-t:-/ alternating with Ø in rf_2 paradigms. This is in keeping with Pike's (1962) trimodal structuring of linguistic units, where each unit has a manifestation.

Thus, the use of Ø in a morphemic formula is required by the

formal criteria of our model; it is justified by the requirements of general statement (see also Allen 1955:112-113). The other alternative would be to use optional tagmemes or different formulae, thus losing the generalization expressed at the hypermorphemic level. Furthermore, as Heller (1964:55) points out, Ø eliminates the need for special symbols to mark tagmemes as optional or obligatory and thus offers an advantage in the machine programming of a natural language. A machine can be directed to select one item from each slot: 'If zero is an item in a slot, the machine may select it; otherwise the machine must take an item which is an available member of that slot'.

It is interesting to note that the + tm slot was filled by an overt marker in Latin. The morphological structure of the Latin perfect was cant-a-v-i, cant-a-v-isti, etc. At an early stage in Vulgar Latin the -v- was lost, leaving the slot it filled empty. This phonetic process may be schematically represented as follows: cant-a-v-i > cant-a-Ø-i, dorm-i-v-i > dorm-i-Ø-i, etc.; hence, the Italian forms /kantáj/, /dormíj/, etc.

The form /kantó/ is the result of a morphophonemic process:

$$\text{mph}_4\text{: + svnuc:/a/} \longrightarrow \text{+ svnuc:/o/ / \_\_ + tm:Ø + pn:Ø}$$

This is considered a morphophonemic process as a result of our definition of the tagmemic morphophonemic level; namely, the level where the fillers undergo any phonetic change. It is interesting to observe that the underlying morphological representation of the third person singular forms is: stem + stem vowel + Ø + Ø. The phonological realization, of course, is simply stem + stem vowel (/kantó/, /temé/, /dormí/).

The morphemic formulae for the past absolute are:

$$\text{past abs rf}_1 = \text{+ vnuc:rf}_1\text{s + svnuc:/a/-/o/}_{\text{mph}4} \text{ + tm:Ø + pn:pn}_5$$

$$\text{past abs rf}_2 = \text{+ vnuc:rf}_2\text{s + svnuc:/e/ + tm:Ø-/t:/ + pn:pn}_5,$$

the /t:/ filler may be used only in the first person singular and the third person singular and plural; the appropriate markers are: /-i/, /-e/, /-ero/

$$\text{past abs rf}_3 = \text{+ vnuc:rf}_3\text{s + svnuc:/i/ + tm:Ø + pn:pn}_5$$

2.5 The present (indicative and subjunctive) paradigms are:

Present Indicative

rf_1	rf_2	rf_3	
/kánto/	/témo/	/dórmo/	'I sing, fear, sleep'
/kánti/	/témi/	/dórmi/	etc.
/kánta/	/téme/	/dórme/	
/kantyámo/	/temyámo/	/dormyámo/	
/kantáte/	/teméte/	/dormíte/	
/kántano/	/témono/	/dórmono/	

Present Subjunctive

/kánti/	/téma/	/dórma/	'that I sing, fear, sleep'
/kánti/	/téma/	/dórma/	etc.
/kánti/	/téma/	/dórma/	
/kantyámo/	/temyámo/	/dormyámo/	
/kantyáte/	/temyáte/	/dormyáte/	
/kántino/	/témano/	/dórmano/	

If the same analytical procedure used in the other tenses is applied to the present, we find that both the + svnuc and + tm slots have no overt markers; they are, therefore, filled by ∅. As was the case for the past absolute, ∅ is required by the formal criteria of our model (i.e., generalization and algorithmic regularity). In oppositional terms, the present is kept distinct from the past absolute in the + svnuc slot: /a,e,i ~ ∅/.

In Latin, the +svnuc slot was overtly marked except for the first person singular of first conjugation verbs: cant-o, cant-a-s, dorm-i-o, etc. In some Italian dialects which have retained final Latin /-s/ in verbal paradigms, the + svnuc slot is overtly marked in second person singular forms: e.g., in Livigno (Piedmont) /kántaš/, /védaš/; in northern Calabria /kántasə/ (see Rohlfs 1969:247). In standard Italian, changes due to vocalic processes and the dropping of final consonants have resulted in the loss of an overt stem vowel marker. In the present indicative forms /kánta/ and /kántano/, it may be justly argued that the /a/ is a stem vowel. However, Old Tuscan forms, such as cantono, lavono, portono, etc. (see Rohlfs 1969:255), contradict this reasoning since they show that at one time the personal ending /-ono/ was the same for all verbal subcategories. There is really no reason to consider the /a/ of these forms as a stem vowel.

Another problem that emerges is the apparent heterogeneity of the personal endings in the indicative. Actually, there are differences in the second person plural and the third person singular and plural. To handle this problem we use the mathematical concept of set partition. In the person-number set for the indicative pn_6, there is a partition X_n, n = 1,2,3 according to the verbal subcategory. Thus, pn_6 = (/-o/, /-i/, ... , /-yamo/, ... , ...), and in the vacant spaces rf_1 verbs have X_1 = (/-a/, /-ate/, /-ano/), rf_2 verbs have X_2 = (/e/, /-ete/, /-ono/), and rf_3 verbs have X_3 = (/-e/, /-ite/, /-ono/)/ The filler for + pn is the union of pn_6 and the partitions: $pn_6 \cup X_n$. In the subjunctive, there are two sets: pn_7 = (/-i/, /-i/, /-i/, /-yamo/, /-yate/, /-ino/) for rf_1 verbs; and pn_8 = (/-a/, /-a/, /-a/, /-yamo/, /-yate/, /-ano/) for both rf_2 and rf_3 verbs.

In rf_3 paradigms, there exist also the so-called inchoative forms:

Indicative	Subjunctive	
/finísko/	/finíska/	'I finish, that I finish'
/finíš:i/	/finíska/	etc.
/finíš:e/	/finíska/	
/finyámo/	/finyámo/	
/finíte/	/finyáte/	
/finískono/	/finískano/	

In these paradigms, the allomorphs /isk-iš:/ overtly mark the present tense, except in the first and second persons plural. The distribution of these allomorphs may be formulated as follows:

$$mph_5\text{: } + \text{tm:/isk/} \longrightarrow + \text{tm:/iš:/} \ / \ \_\_ \ /e,i/$$

Thus, in the present tense, rf_3 verbs are subdivided into rf_{3a} for non-inchoative stems and rf_{3b} for inchoative ones.

There is also an accent shift similar to the one for the imperfect. Thus, rf_1, rf_2, and rf_{3a} verbs have the following rule:

mph_6: for any pres ind or subj rf_1, rf_2, or rf_{3a} verbs,
+ vnúc + svnuc + tm + pn $\longrightarrow$ + vnuc + svnuc + tm + pń,
when pn = (/-yamo/, /-ate/, /-ete/, /-ite/, /-yate/)

For rf_{3b} verbs the appropriate rule is:

mph_7: for any pres ind or subj rf_{3b} verb,
+ vnuc + svnuc + tḿ + pn $\longrightarrow$ + vnuc + svnu- + tm + pń,
when pn = (/-yamo/, /-ite/, /-yate/)

The morphemic formulae for the present are:

pres ind rf_1 = + vnuc:rf_1s + svnuc:∅ + tm:∅ + pn:$pn_6 \cup X_1$

pres ind rf_2 = + vnuc:rf_2s + svnuc:∅ + tm:∅ + pn:$pn_6 \cup X_2$

pres ind rf_{3a} = + vnuc:rf_{3a}s + svnuc:∅ + tm:∅ + pn:$pn_6 \cup X_3$

pres ind rf_{3b} = + vnuc:rf_{3b}s + svnuc:∅ + tm:∅-/isk-iš:/$_{mph5}$
+ pn:$pn_6 \cup X_3$, where + tm:∅ is used only
when pn = (/-yamo/, /-ite/)

pres subj rf_1 = + vnuc:rf_1s + svnuc:∅ + tm:∅ + pn:pn_7

pres subj rf_2 = + vnuc:rf_2s + svnuc:∅ + tm:∅ + pn:pn_8

pres subj rf_{3a} = + vnuc:rf_{3a}s + svnuc:∅ + tm:∅ + pn:pn_8

pres subj rf_{3b} = + vnuc:rf_{3b}s + svnuc:∅ + tm:∅-/isk-iš:/$_{mph5}$
+ pn:pn_8, where + tm:∅ is used only
when pn = (/-yamo/, /-yate/, /-ite/)

3. There exist two excellent structuralist (Agard and Di Pietro 1965:9-13; Hall 1971:84-108) and transformational (Costabile 1973; Devine 1968) descriptions of the regular finites. In structuralist approaches, the stem vowel is considered part of the verb stem. However, this analysis ignores the fact that a stem vowel unit may be factored out for the imperfect, future, conditional, and past absolute; it thus misses a generalization. Furthermore, in structuralist works, the future and conditional stems become /kante-/ for rf_1 verbs which implies a stem change and thus is counterintuitive to the concept of regularity. The other alternative would be to exclude the future and

conditional from the regular finite paradigms. Similarly, an ad hoc description for the present is adopted since the forms /témo/, /dórmo/, etc. appear to have lost part of their stem, namely the stem vowel. The solution is that the stem vowel is cancelled before vowel-initial endings—a solution, incidentally, which gives unit status to the stem vowel. Furthermore, one would expect the stem vowel to be cancelled in the past absolute forms /kantáⁱ/, /dormíⁱ/, etc.; but, of course, this is not the case.

Some diachronic evidence may be added in support of an autonomous stem vowel marker. The Latin perfect forms for the second conjugation show tim-u-i, tim-u-isti, etc. To trace the /e/ of the corresponding Italian forms /temeⁱ/, /temésti/, etc. back to Latin -u- would be historically misguided. The only explanation of the Italian /e/ in place of Latin -u- is the tendency for regularization: i.e., by the process of analogy the /e/ replaces -u- because the former vowel functions as a regular marker in the verbal structure of the regular finites.

The transformational treatment of Costabile (1973:27-37) is identical to the structuralist ones; i.e., the stem vowel is again part of the verb stem. Costabile treats the loss of this vowel by a deletion rule, and in so doing also gives it an autonomous status. Devine (1968), on the other hand, considers the stem vowel to be a separate unit. However, the model she proposes fails to predict the sequence of two null allomorphs found in our analysis; therefore, some cases are treated separately and a generalization about the underlying morphological structure of the regular finites is lost in the process.

4. In conclusion, the main theoretical consideration that arises from our analysis of the Italian regular finites is that the tagmemic model allows us to abstract the underlying structure common to the various moods and tenses of this specific verbal subclass. It provides, as a corollary, a mechanical device or algorithm which generates the various paradigms. Consequently, this model removes the 'ad hocness' which would otherwise be used for the description of certain tenses.

In the tagmemic model that we propose, the general or canonical statement is called the hypermorphemic formula. The term hypermorphemic, which is also used in stratificational models, indicates a high level of abstraction. Actual slot-filling occurs at the morphemic level. Variations in the phonetic shape of the fillers are handled by process rules (as found in transformational studies) at the morphophonemic level. This tripartite model of word construction is eclectic in the sense that it combines process rules with tagmemic constructional techniques. The output (word) of the simultaneous three-level layering is a class unit which subsequently fills slots at the higher phrase level.

In any descriptive survey of a particular language, the problem of artifact versus reality always emerges. It is clear that any linguistic model such as ours is artificial in the sense that there is no reason for believing that it corresponds to psychological reality. The current belief in transformational circles that linguistics is a subfield of psychology is interesting but lacks convincing empirical data. It is believed that linguistic models (i.e., transformational ones) are not mere descriptive conveniences but possess psychological

reality (see Dingwall 1971:759-760). Perhaps the study of aphasia and other speech disorders may provide clues as to the nature of the so-called mental grammar (see Whitaker 1971). However, the conclusion that is generally reached by most linguists is that the structure of language is, to a certain degree, a creation of the analyst, generally invented to fit the prerequisites of his particular orientation.

In our opinion, the best way to eliminate or at least attenuate the chance of biasing analytical results is by choosing whatever methodology best describes a particular aspect of a specific language. For Italian, the tagmemic model of word construction (together with process rules) appears to be the most theoretically satisfying one for describing the verbal subclass known as the regular finites.

We conclude by pointing out that, as Labov (1971:481) has emphasized, it is through the convergence of several techniques that progress in the common goal of discovering the nature of language can be made. By so doing, we will be able to eliminate the common failing we all share as linguists and which Labov expresses so well: 'We try too hard to prove ourselves right'.

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THE GENITIVE CASE IN AZARBAYJANI: A PROBLEM OF RECURSIVENESS

Reza Ordoubadian
Middle Tennessee State University

The genitive case in Azarbayjani is realized through a series of transformations of pronominal enclitic structures and the verbal /var-/, one of the forms of the reconstructed infinitive */E-max/ (to be, to exist). According to the theory of sentence structure in Azarbayjani--discussed elsewhere--a simple sentence will yield the following strings:

$$S \longrightarrow NP + VP$$
$$NP \longrightarrow S_1 + NP$$
$$S_1 \longrightarrow NP_1 + \text{pronominal enclitic (Pro-Enc)},$$

realizing the deep structure $\underline{/}NP_1$ + Pro-Enc + NP + V$\underline{P/}$. In order to realize a surface structure a T-Pro-Enc must transform the pronominal enclitic finally, yielding $\underline{/}NP_1$ + NP + VP + Pro-En$\underline{c/}$.

Pronominal enclitic structures are generated by a conjugation of the hypothetical verb */E-max/ as seen in the following paradigm:

	singular		plural		
1st person	-æm	(am)	-ux	(are)	
2nd	-sæn	(art)	-suz	(are)	
3rd	-dI,I	(is)	-I+lar	(are)	(lar is the plural maker).

Pronominal enclitics are the most stable constructs in Azarbayjani sentence structures, and through various transformations they are used both in NP and VP structures.

Since transformations of pronominal enclitics constitute a part of the genitive structure, a simple, non-genitive use of a pronominal enclitic will provide a good point of departure in understanding the structure of the genitive case in Azarbayjani. When analyzed, the sentence /mæn + jan-a + dE + ær + æm / (I tell John) will yield the following derivations:

$S \longrightarrow NP + VP$

$NP \longrightarrow S_1 + NP$

$S_1 \longrightarrow NP_1 + V_{E\text{-}max}$

$NP_1 \longrightarrow$ mæn (I)

$V_{E\text{-}max} \longrightarrow$ æm (am - 1st person singular marker)

$NP \longrightarrow$ jan + a

jan $\longrightarrow$ John

-a $\longrightarrow$ dative marker

$VP \longrightarrow$ verb stem + Aux

Verb Stem $\longrightarrow$ dE- (tell)

Aux $\longrightarrow$ Aspect + Tense

Aspect $\longrightarrow$ -ær- (habitual marker)

Tense $\longrightarrow$ Ø (present marker),

yielding /mæn + -æm + jan +-a + dE -+-ær-/. With the application of T-Pro-Enc this string will yield the surface structure /mæn + jan +-a + dE - +-ær- +-æm/. This generalization concerning the position of the pronominal enclitics will hold true except when the verbal form /var-/ constitutes the verb stem. In this case instead of shifting finally, the pronominal enclitics will undergo a transformation to generate possessive enclitics before generating possessive phrases. Simple enclitics will undergo the following transformations for possessive constructions:

-æm + poss $\Longrightarrow$ -Im	-ux + poss $\Longrightarrow$ -mux
-sæn + poss $\Longrightarrow$ -In	-suz + poss $\Longrightarrow$ -uz
-I + poss $\Longrightarrow$ -In	-I + lar + poss $\Longrightarrow$ lar + I.

The possessive enclitics thus derived are repeated recursively after both the possessor and the objects of possession. In English an 's morpheme or an "of" phrase indicates the possession; however, in Azarbayjani the process is exceedingly complex. There are two basic constructs: 1) a single possessor-possessed relationship such as /mæn + -Im + kItab + -Im/ (my book, book of mine), and 2) a series of possessed-possessor relationships such as /mæn + -Im + kItab + -Im + -In + sæfae(s) + -In/ (the page of my book).

1. The simple possessor-possessed relationship is established by repeating the possessive enclitic both after the possessor and the object of possession as if saying I+my + book + my. For example,

the sentence /mæn + -Im + kItab + -Im + var / (I have / a / book) has the following derivations:

$$S \longrightarrow NP + VP$$

$$NP \longrightarrow S_1 + NP$$

$$S_1 \longrightarrow NP_1 + V_{E\text{-}max}$$

$$NP_1 \longrightarrow \text{mæn (I)}$$

$$V_{E\text{-}max} \longrightarrow \text{-æm (am)}$$

$$NP \longrightarrow \text{kItab (book)}$$

$$VP \longrightarrow \text{var (is, exists)},$$

so that the string /mæn + æm + kItab + var/ is realized, which through T-Poss (/-æm + poss $\Longrightarrow$ -IM/) results in the string /mæn + Im + kItab + var /. However, as the example shows, the morpheme /-Im/ is obligatorily repeated after both /mæn/ and /kItab/, so that the second /Im/ has to be accounted for. This is an indication that this sentence has a double base construct, and a theory formulated in my larger work on Azarbayjani leads me to believe that the two bases for this construct are

a. /mæn + -æm + var/ $\Longrightarrow$ /mæn + -Im + var/ (I have)

b. /kItab + var/ (book exists),

resulting in the string /mæn + -Im (kItab + var) + var /, from which through a transformation deletion (identical verbs) and a transformation addition (Im) the surface structure /mæn + -Im + kItab + -Im + var/ is realized. It is possible to assume a slightly different structure for the second base sentence; however, the result will yield the same abstract string eventually.

2. A series of possessor-possessed constructs are a result of a complex set of sentence generation and transformations dealing with addition, deletion, and metamorphosis. In a complex set each element which is an object of possession and a possessor of another object generates its own possessive enclitic independently. The s of the first object of possession generates two possessive enclitics: one relating to the head possessor noun, very much as in (1), and another relating to itself being a head noun possessing a following noun. This second element is always in the form of third person enclitic because in the deep structure of each series a subject 0 (that, it) does exist which creates its own pronominal enclitic which transforms to a possessive enclitic. For example, from a construct such as /mæn + -Im + kItab+-Im + -In + sæfæ (s) +-In+-In + xætt + -In / (the line of the page of my book) the following string is realized:

/mæn + -æm + poss + kItab + mæn + -æm + poss + o + -dI + poss + sæfæ (s) + o + -dI + poss + o + -dI + poss + xætt + o + -dI + poss /. This string is a result of the following derivations:

#NP $\longrightarrow$ S_1 + S_2 + S_3 + S_4 #

S_1 $\longrightarrow$ NP + $V_{E\text{-}max}$

NP $\longrightarrow$ mæn (I)

$V_{E\text{-}max}$ $\longrightarrow$ -æm (am)

(A) -æm + poss $\Longrightarrow$ -Im (my)
result: /mæn + -Im / (I, my)

S_2 $\longrightarrow$ NP + S_5 + S_6

NP $\longrightarrow$ kItab (book)

S_5 $\longrightarrow$ NP + $V_{E\text{-}max}$

NP $\longrightarrow$ mæn (I)

$V_{E\text{-}max}$ $\longrightarrow$ -æm (am)

(B) -æm + poss $\Longrightarrow$ -Im
result: /mæn + -Im / (I, my)

S_6 $\longrightarrow$ NP + $V_{E\text{-}max}$

NP $\longrightarrow$ o (it-third person singular marker)

$V_{E\text{-}max}$ $\longrightarrow$ -dI (is)

(B_1) -dI + poss $\Longrightarrow$ -In (its)
result: /o +(n)In/ (it + its)

Result for S_2: /kItab + mæn + -Im + o +(n)In /
(book+I+my+it + its)

S_3 $\longrightarrow$ NP + S_7 + S_8

NP $\longrightarrow$ sæfæ (page)

S_7 $\longrightarrow$ NP + $V_{E\text{-}max}$

NP $\longrightarrow$ o (it)

$V_{E\text{-}max}$ $\longrightarrow$ -dI (is)

(C) -dI + poss $\Longrightarrow$ -In (its)
result: /o + (n)In/ (it + its)

$S_8 \longrightarrow NP + V_{E\text{-}max}$

$NP \longrightarrow o$ (it)

$V_{E\text{-}max} \longrightarrow$ -dI

(C_1) -dI + poss $\Longrightarrow$ -In (its)
result: /o + (n)In/ (it + its)

Result for S_3: /sæfæ + o + (n)In + o + (n)In/
(page + it + its + it+its)

$S_4 \longrightarrow NP + S_9$

$NP \longrightarrow$ xætt (line)

$S_9 \longrightarrow NP + V_{E\text{-}max}$

$NP \longrightarrow o$ (it)

$V_{E\text{-}max} \longrightarrow$ -dI (is)

(D) -dI + poss $\Longrightarrow$ -In, -I (its)
result: /o + (n)In/ (It + its)

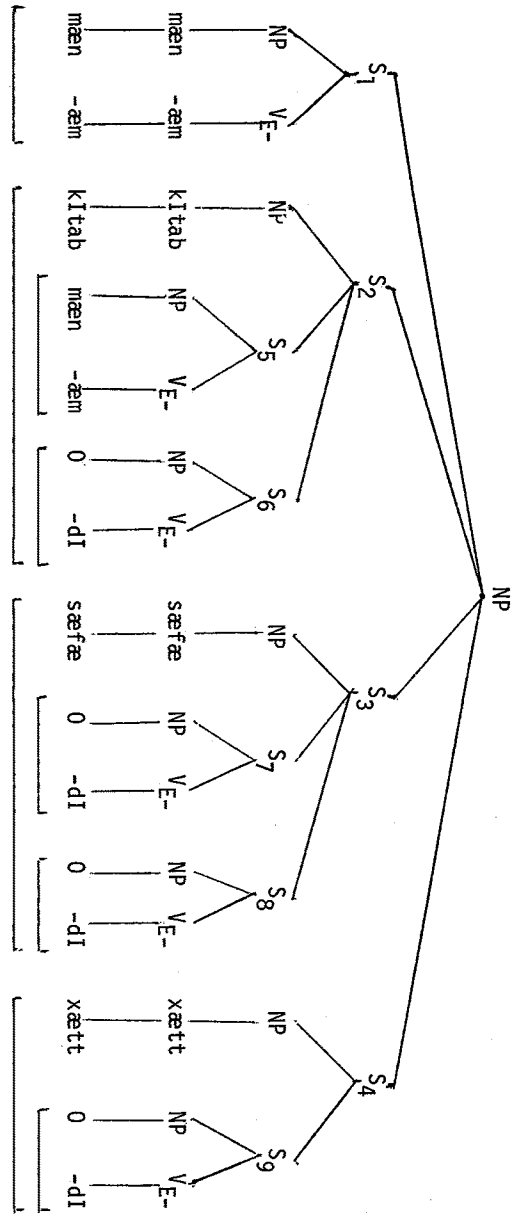
Result for S_4: /xætt + o + (n)In/ (line + it + its)

Total result: /(mæn+-Im) + (kItab + mæn+-Im + o + nIn) +
(sæfæ + o + nIn + o + nIn) + (xætt+ o + nIn)/
(See Diagram I)

In order to realize the surface structure from this complex system T-Poss must be applied six times to S_1A, S_2B and B_1, S_3C and C_1, and finally S_4D. Also, all /o/ (it) morphemes are deleted by applying T-Del four times.

The process of genitivization in Azarbayjani requires, therefore, that after initially establishing a possessor-possessed relationship between the head noun and the first object of possession in the series, the rest of the elements become possessed by one nearest to the left of the noun, thus generalizing each possessor with an /o/ (it) morpheme. Possessive affixes thus generated from the simple pronominal enclitics and sentence series follow each noun in possession in each possessive noun cluster of morphemes.

In analyzing the genitive case three points are to be considered: 1) both the head noun and the object of possession require enclitic affixes generated through a series of expansion and transformations of the NP of a sentence, 2) because person and number constrain any Azarbayjani structure, multiple transformations are necessary for agreement, and 3) since vowel harmony plays an important role in the



-æm + poss⇒Im -æm + poss⇒-Im -dI + poss⇒In -dI + poss⇒In -dI + poss⇒-In -dI + poss⇒-In

mæn ⇒ ∅ o⇒∅ o⇒∅ o⇒∅ o⇒∅

[mæn + Im kItab+Im+In+sæfæ(s)+In+In+xætt+In]

DIAGRAM I

Azarbayjani phonology, morphophonemic variations must be accounted for in the genitive case, as in any other structure in the language. The chart in Appendix A shows the variations in the Tabrizi dialect, the dialect that has provided the main source of my data. A slight variation in the Baku dialect and Zanjani dialect could be discerned that is unimportant for the purposes of this paper.

In addition to the conclusions discussed earlier, the study of Azarbayjani more and more convinces me that a simple sentence, at least in this language, exists only theoretically and that any simple sentence in Azarbayjani could be immediately analyzed into a NP and an S_1, which is a sentence with a noun and a form of the verb */E-max/, so that it could be generalized that there is only one type of basic sentence in Azarbayjani, S_1 type, and that the rest are expansions and transformations of this basic type. The recursive process in the genitive case in Azarbayjani points to this fact.

APPENDIX A

A vowel in the first syllable determines the quality of the succeeding vowels as the following context-sensitive derivations indicate:

1. $a \longrightarrow æ \Big/ \begin{Bmatrix} i \\ æ \\ u \\ e \end{Bmatrix} -$

2. $æ \longrightarrow a \Big/ \begin{Bmatrix} I \\ u \\ o \end{Bmatrix} -$

3. $I \longrightarrow i \Big/ \begin{Bmatrix} æ \\ i \end{Bmatrix} -$

4. $i \longrightarrow I \Big/ \begin{Bmatrix} a \\ I \\ o \end{Bmatrix} -$

5. $\begin{Bmatrix} i \\ I \end{Bmatrix} \longrightarrow u \Big/ \begin{Bmatrix} u \\ e \end{Bmatrix} -$

6. $i \longrightarrow u/u \quad -$

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RELATIVIZATION IN BANTU LANGUAGES REVISITED*

Eyamba G. Bokamba
Department pf Linguistics
University of Illinois
at Urbana-Champaign

1.0 Introduction

The immediate concern of this paper is to provide an analysis of restrictive relative clause formation in some Bantu languages focusing on data from two of these languages: Dzamba (a Gunthrie (1948) Zone C language), and Swahili (Zone G language). The central questions to which the paper addresses itself are of two types: (1) the lack of grammatical agreement between the verb of the embedded relative clause and its subject in the Dzamba-type languages, but not in the Swahili-type; and (2) the apparent obligatory subject postposing in object relativization in the languages of both types.

1.1 The data. Specifically, relative clauses in Dzamba, Likila, Lingala, and Lomongo (hereafter Dzamba-type languages) have the structure illustrated by the Dzamba sentences (1) and (2):[1]

(1) a. o-mo-to ó-kpa-áki i-mu-ndondo a-kim-í.
the-person RM-take-ed the-jug he-flee-I.Pst.
(The person/man who took the jug just fled.)

b. *o-mo-to (ó)-a-kpa-áki i-mu-ndondo a-kim-i.
the-person (RM) -he-took the-jug he-fled

c. ba-ba-to bá:-kpa-áki i-mu-ndondo ba-kim-i.
the-people RM-take-ed the-jug they-fled
(The people who took the jug just fled.)

d. *ba-ba-to (bá)-ba-kpa-áki i-mu-ndondo ba-kim-i.
the-people (RM) -they-took the-jug they-fled

(2) a. i-mu-ndondo í-mu-tom-áki Poso loome mu-bung-i.
the-jug RM-Ag-send-ed Poso today it-lose-I.Pst.
(The jug which Poso sent today is lost.)

b. *i-mu-ndondo Poso a-tom-áki loome mu-bung-i
the-jug Poso she-sent today it-lost

c. *i-mu-ndondo Poso a-mu-tom-áki loome mu-bung-i.
the-jug Poso she-it-sent today it-lost

d. *i-nu-ndondo í-mu-tom-áki loome Poso mu-bung-i.
the-jug RM-Ag-sent today Poso it-lost

The second group of Bantu languages, which includes Swahili, Kihung'an, and Tshiluba (hereafter the Swahili-type languages), has relative clauses similar to those illustrated above but differs from them in some important respects as may be observed from the Swahili sentences (3) and (4):

(3) a. m-toto a-li-ye-kuta ki-tabu ch-angu leo a-me-jificha.
child he-ed-RM-find book Ag-my today he-Perf-hide
(The child who found my book today is hidden/hiding.)

b. *m-toto ye-a-li-kuta ki-tabu ch-angu leo a-me-jificha.
RM-he-ed-find book my today he-is hiding

c. *m-toto ye-li-kuta ki-tabu ch-angu leo a-me-jificha.
RM-ed-find book my today he-is-hiding

(4) a. ki-tabu a-li-cho-ki-kuta m-toto leo ki-me-potea tena.
he-ed-RM-it-find it-Perf-lose again
(The book that the child found today is lost again.)

b. *ki-tabu m-toto a-li-cho-ki-kuta leo ki-me-potea tena.
the-book the child he-ed-RM-find today it-is-lost again

c. *ki-tabu a-li-cho-ki-kuta leo m-toto ki-me-potea tena.
he-ed-RM-it-find today child it-is-lost

These sets of sentences illustrate two basic problems in the formation of relative clauses in Dzamba, Swahili, and related languages. The first problem is that in Dzamba and Dzamba-type languages, but not in Swahili-type, there is no grammatical agreement between the verb of the embedded relative clause and its logical subject noun phrase. In particular, the agreement or subject prefixes for third person singular and plural for the human class (1/2) in Dzamba and most other Bantu languages are {a-} and {ba-}, respectively; they are not [ó-] and {bá-} as illustrated in the Dzamba sentences (1a,c). Assuming that the subject prefixes in the Dzamba-type languages, as in the Swahili-type, cooccur with the relative markers, what we expected from the relativization of the subject noun phrase in the structures underlying (1a) and (c) above are sentences such as (1b) and (1d). But as their ill-formedness shows, these sentences are not possible in Dzamba.

1.2 Word Order in Bantu Languages. The second problem is that the subject noun phrase of the embedded relative clause in both types of languages, as can be seen from the Dzamba sentences in (2) and the Swahili in (4) above, must occur immediately after, but not before, its verb. This apparent subject-postposing phenomenon that I shall refer to as Subject-Verb Inversion is characteristic of object relativization in the languages under consideration here; it never occurs in subject relativization. The agreement pattern exemplified in the Dzamba relative clauses and the word order resulting from the Subject-

Verb Inversion phenomenon are characteristically deviant, because in almost all Bantu languages every finite verb must agree in number, person, and class-gender with its subject, and the basic word order is SVO (subject-verb-object). The question to which I am addressing myself here is why the above sentences have the surface structures they do.

2.0 Relative Clause Formation

2.1 Subject Relativization. To answer this question, let us consider first the case of subject relativization in both languages. Assume that the rules of Subject-Verb Agreement in Bantu languages is as in (5), and that Relative Clause Formation is as in (6) as formulated by Ross (1967: 101):

(5) Subject-Verb Agreement (obligatory)

$$_{Z}[\ NP_1 \quad \# \quad _{S}[\ NP_2 \quad V \quad X\ ] \quad \# \quad Y]$$

$$1 \quad 2 \quad 3 \quad 4 \quad 5 \quad 6 \quad 7 \quad \xRightarrow{\text{oblig}}$$

$$\begin{bmatrix}\alpha\ \text{class}\\ \beta\ \text{pers}\end{bmatrix}$$

$$1 \quad 2 \quad 3 \quad 4 \quad 5 \quad 6 \quad 7$$

$$\begin{bmatrix}\alpha\ \text{class}\\ \beta\ \text{pers}\end{bmatrix} \begin{bmatrix}\alpha\ \text{class}\\ \beta\ \text{pers}\end{bmatrix}$$

(6) Relative Clause Formation (obligatory)

$$W - {}_{NP}[\ NP - [_{S}X - NP - Y]_{S}]NP - Z$$

$$1 \quad 2 \quad 3 \quad 4 \quad 5 \quad 6 \quad \xRightarrow{\text{oblig}}$$

$$1 \quad 2 \quad 4\ \#[3 \quad 0 \quad 5] \quad 6$$

Condition: 2=4

Assume that the structure underlying a sentence such as the Dzamba (1a) above is roughly (7a), and that the rule of Subject-Verb Agreement (5) is extrinsically ordered to precede the Rule of Relative Clause Formation (6), the application of these rules will give us the derivation in (7):

(7) a. o-mo-to $_{S}$(o-mo-to -kpa-áki i-mu-ndondo)$_{S}$ -kim-i
the person the person took the-jug fled

b. Agreement: o-mo-to $_{S}$(o-mo-to a-kpa-áki imundondo)$_{S}$ a-kimi
the person the person he-took the jug he-fled

c. Relativization: o-moto $_S$(Ø ó-a-kpaáki imu-ndondo)$_S$ a-kimi.
the-pers. RM-Ag-took the-jug he-fled

d. Output: *omo-to ó-a-kpaáki imu-ndondo a-kim-i.
the person RM-he-took the jug he-fled

As the ungrammaticality of (7d) demonstrates, this order of application of the rules cannot yield the correct output. If the rules are applied in the reverse order, namely, Relative Clause Formation (6) before Subject-Verb Agreement (5), the resulting sentence will be grammatical as may be seen from (8d):

(8) a. omo-to $_S$(omo-to -kpaáki imu-ndondo)$_S$ -kimi

b. Relativization: omo-to $_S$(Ø ó-kpaáki imu-ndondo)$_S$ -kimi
RM-took the-jug

c. Agreement: omo-to $_S$(Ø ó-kpaáki imu-ndondo)$_S$ a-kimi
RM-took the-jug he-fled

d. Output: omo-to ó-kpa-áki imu-ndondo a-kim-i.
(The person who took the jug fled.)

This derivation suggests two things: (1) that the correct order of application of the rules stated in (5) and (6) must be Relative Clause Formation before Subject-Verb Agreement; and (2) that Relativization does not apply in the embedded clause presumably because of the deletion of the subject NP under relativization.

If these conclusions are correct, we expect to find the same pattern of agreement when a non-human subject NP is relativized; but as can be seen from (9a) this is not the case:

(9) a. izi-kenge í-zi-bung-i o kalasi zi-ba-áki za-nga.
the-slate RM-Ag-lost at school it-be-ed of-my
(The slate that is lost at school was mine.)

b. *izi-kenge i-bung-i o kalasi zi-ba-áki za-nga.
the-slate RM-lost

c. *izi-kenge zi-bung-i o kalasi zi-ba-áki za-nga.
the-slate it-lost

d. ma:-kenge má:-bung-i o kalasi ma-ba-áki ma-nga.
the-slates RM-lost at school they-were mine
(The slates that are lost at school were mine.)

e. *ma:-kenge ma-bung-i o kalasi ma-ba-áki ma-nga.
the-slates they-lost at school they-were mine

The embedded verb of both sentence (9a) and (9d), as we shall show in a moment, do contain an agreement prefix each. These sentences appear to be derived by extrinsically ordering Agreement before Relativization as in derivation (10), but not by the converse ordering given in derivation (11):

(11) a. izi-kenge $_S$(izi-kenge -bungi o kalasi)$_S$ -ba-áki zanga

b. Agreement: izi-kenge $_S$(izi-kenge zi-bungi o kalasi)$_S$ zi-ba-áki zanga
it-lost it-was

c. Relativization: izi-kenge $_S$(Ø í-zi-bungi o kalasi)$_S$ zi-ba-aki zanga
RM-Ag-lost

d. Output: izi-kenge í-zi-bungi o kalasi zi-ba-áki zanga.
(The slate that is lost at school was mine.)

(12) a. Relativization: izi-kenge $_S$(Ø í-bungi o kalasi)$_S$ -ba-áki...
RM-lost

b. Agreement: izi-kenge $_S$(Ø í-bungi o kalasi)$_S$ zi-ba-áki ...
RM-lost it-was

c. Output: *izi-kenge i-bungi o kalasi zi-ba-áki zanga.

That is, if the Agreement rule stated in (5) above applies cyclically, its applicational precedence over Relativization (6) will yield the desired output; but if the ordering of the two rules is reversed as in (12), Relativization will bleed Agreement in the first cycle by deleting the embedded Subject noun phrase.

In view of these facts, we cannot appeal to rule ordering to resolve the problem of subject-verb agreement exemplified in the Dzamba sentences (1a,c) and (2a) above, in contrast to sentences such as (9a). In Swahili, as will be recalled from sentences (3)-(4), every verb agrees in number, person, and class-gender with its logical subject noun phrase; thus the problem of agreement does not arise at all. Since rule ordering cannot be appealed to for an explanation of the facts of subject relativization in Dzamba, the only remaining alternatives are to either assume that relativization involving human noun phrases in this language and related ones is an exception to the obligatory rule of Subject-Verb Agreement, or that the rule does in fact apply as in all other cases but that there is a neutralization of the subject prefixes when such noun phrases are relativized. This process of neutralization may be the result of vowel assimulation or some type of deletion.

If the first alternative is chosen, there is little else that can be said and the problem will remain unresolved. This leaves us with the second alternative. To argue that Subject-Verb Agreement applies in all cases including instances where [+ human] noun phrases are relativized, one must show that there are certain specified contexts where such an agreement is not neutralized. Sentences involving negated verbs in Dzamba are such as context. Consider in this respect the negative variants of sentences (1a,c) and (9a,d) above given below as (13a,b) and (14a,b), respectively:

(13) a. o-mo-to ó-ta-ò-kpa-áki imu-ndondo emba a-kimi.
the-pers RM-Neg-?-took the-jug not he-fled
(The person who did not take the jug fled.)

b. ba-bato bá-ta-bà-kpa-áki i-mundondo emba ba-kim-í.
the-persons RM-Neg-Ag-took the-jug not they-fled
(The persons who did not take the jug fled.)

c. *o-moto ó-ta-ó-kpa-áki i-mundondo emba a-kim-í.
RM-Neg-RM-took

d. *o-moto ó-ta-kpa-áki i-mundondo emba a-kim-í.
RM-Neg-took

e. *o-moto ó-ta-a-kpa-áki i-mundondo emba a-kim-í.
RM-Neg-he-took

f. *ba-bato bá-ta-kpa-áki i-mundondo emba ba-kim-í.
RM-Neg-took

g. *ba-bato bá-ta-bá-kpa-áki i-mundondo emba ba-kim-í.
RM-Neg-RM-took

(14) a. i-zikenge í-ta-zì-bung-áki o kalasi emba zi-ba-áki zanga.
RM-Neg-Ag-lose-ed not it-was
(The slate which was not lost at school was mine.)

b. ma:-kenge má-ta-mà-bung-áki o kalasi emba ma-ba-áki manga.
RM-Neg-Ag-lose-ed not they-were
(The slates which were not lost at school were mine.)

c. *ma:-kenge má-ta-bung-áki o kalasi emba ma-ba-áki manga.
RM-Neg-lose-ed

These sentences illustrate three important facts of relativization in Dzamba. The first fact, which I have alluded to all along this discussion, is that the relative marker (RM) and the prefix immediately following it in sentences such as (9a) above cannot be considered as a single morpheme: they are two separate morpheme. That this must be the case is shown by the occurrence of the negative marker {-ta-} between the relative marker {í-} in (14a) and the next prefix on that verb, viz., {-zi-}. Similarly, in (14b) the negative marker occurs between the relative marker {má-} and the third prefix {-ma-}.

The second fact is that the non-occurrence of sentences such as (13c,g) and (14d) establishes once for all, what I have suspected all along, that the prefix which occurs immediately after the relative marker, for instance {-zi-} in (9a), is an agreement prefix. Hence the morphemes {-zi-} and {-ma-} in (14a,b) are subject prefixes. And this brings us to our third fact: there is a neutralization of agreement prefixes in subject relativization (thus far) when a human noun phrase is relativized. This fact is illustrated by the occurrence of the prefixes {ò} in (13a) and {-bà-} in (13b). That these subject prefixes must have been neutralized is clearly demonstrated by the ill-formedness of sentences such as (13d,f) where these prefixes are omitted.

The occurrence of the low-toned prefix {-ò-} in (13a) appears very odd at this point. If the concordial agreement we have discussed here with respect to sentences such as (9a,d), (13b), and (14b) is derived in

accordance with the Subject-Verb Agreement rule stated in (5), the sentence we expect as the negative variant of (1a) is the ungrammatical (13e) where the subject prefix is {à-}, instead of {ò-}. But as (13e) clearly shows, such a sentence is not permissible. These sentences, viz. (13a) and (13e), raise the question of whether the agreement prefixes found in the other sentences discussed thus far is in fact derived according to rule (5). Since the constructions examined thus far have involved identical subject noun phrases, this question cannot be answered. To answer it we need to consider examples in which the subject of the embedded clause differs from that of the main clause. Constructions involving object relativization are precisely what we need; we therefore, consider them next.

2.2 Object Relativization. In addition to the problem of agreement discussed above, object relativization in Dzamba, Likila, and Lingala characteristically involves an obligatory subject-verb inversion phenomenon in the embedded relative clause. This phenomenon which is reminiscent of Subject-Auxilliary inversion in English, for example, also occurs obligatory in three other Bantu languages: Swahili (Zone G), Kihung,an (Zone H), and Tshiluba (Zone L). I shall refer to these languages as the Swahili-type, in contrast to the Dzamba-type, for reasons that will become apparent in the course of the analysis. I beg n the examination of the Dzamba-type object relativization first, then I take up that of the Swahili-type languages.

2.2.1 Object Relativization and Inversion. Relative clauses in which an object noun phrase is relativized in the Dzamba-type languages have the structure illustrated by the Dzamba sentence (15c):

(15) a. oPoso a-tom-áki mu-ndondo loome.
(Poso sent a jug today.)

b. i-mu-ndondo mu-bung-í.
(The jug is lost.)

c. i-mu-ndondo í-mu-tom-áki Póso loome mu-bung-í.
the-jug RM-Ag-send-ed Póso today it-lose-I/Pst.
(The jug which Poso sent today is lost.)

d. *i-mu-ndondo Póso a-tom-áki loome mu-bung-í.
the-jug Póso she-sent today it-lost

e. *i-mu-ndondo Poso a-mu-tom-áki loome mu-bung-í.
the-jug Poso she-Ag-sent today it-lost

f. *i-mu-ndondo í-mu-tom-áki loome Póso mu-bung-í.
the-jug RM-Ag-sent today Póso it-lost

These sentences show that given a simple sentence such as (15a) where the deep subject noun phrase oPoso precedes its verb and the direct object noun phrase mundondo follows the verb, the relativization of the latter will yield sentence (15c) where the subject noun phrase of the embedded relative clause must follow its verb. That this word order must be strict-

ly observed is attested by the ungrammaticality of (15d,e) where the logical subject noun phrase of the embedded clause precedes its verb, and that (15f) where it follows the adverb loome. Notice that as in the previous examples of relativization, the verbs contain a relative marker and an agreement prefix; but this prefix agrees with the head noun phrase imundondo, rather than with the deep subject, oPoso. Put differently, the verbs of the embedded relative clause and the main clause agree in number and class-gender with the head noun phrase in (15c). This is a peculiar, and yet an interesting, agreement pattern in the Dzamba-type languages.

If the facts given in (15) are correct, then clearly the subject prefixes observed in those constructions involving subject relativization cannot be agreement prefixes for the embedded subject noun phrase as I previously suggested. Consider in this respect the relativization of the object noun phrases of a sentence such as the Dzamba (16a):

(16) a. oPoso a-eza-áki ba-butu zi-bata loome.
Poso Ag-gave the-guests a-duck today
(Poso gave the guests a duck today.)

b. i-zi-bata i-zi-eza-áki oPoso ba-butu loome zi-kim-í.
RM-Ag-gave Poso it-fled
(The duck Poso gave the guests today ran away.)

c. *i-zi-bata oPoso (í)-a-eza-áki ba-butu loome zi-kim-í.
the-duck Poso (RM)-she-gave the-guests today it-fled

d. i-zibata í-zi-eza-áki ba-butu oPoso loome zi-kim-í.
(The duck that the guests gave Poso today ran away.)

e. *i-zibata í-zi-eza-áki ba-butu loome oPoso zi-kim-í.
RM-Ag-gave the guests today Poso

f. ba-butu bá:-eza-áki oPoso zi-bata loome ba-keí.
the-guests RM-gave Poso a duck today they-left
(The guests to whom Poso gave a duck today left.)

g. *ba-butu a-eza-áki oPoso zi-bata loome ba-keí.
she-gave Poso they-left

h. *ba-butu bá-eza-áki zi-bata oPoso loome ba-keí.
RM-gave a duck Poso

As expected, these sentences show that the verb of the embedded relative clause must agree with the head noun phrase, and that the subject of the embedded clause must occur immediately after its verb. In particular, when the direct object noun phrase i-zi-bata is relativized, the verb of the main clause and that of the embedded clause must agree with the head noun, i-zi-bata (16b) as the ill-formedness of (16c) shows. Word order-wise, when the direct object is relativized, the embedded subject noun phrase must immediately follow its verb, it cannot, for example, occur either before its verb or where the deleted direct object noun phrase was as the inappropriateness of (16c,d) attests. (16d) would be completely

ungrammatical if the NP ba-butu 'guests' is replaced by a non-human NP. Note further with respect to word order that the embedded subject NP cannot occur after the adverb loome 'yesterday' as the ill-formedness of (16e) demonstrates. The same agreement pattern and word order restrictions apply to the relativization of an indirect object NP as sentence (16f), in contrast to (16g,h) shows. It should be pointed out here that sentence (16f) can be ambiguous if the NP oPoso is not stressed; in this case it would be interpreted that the guests were the ones who gave the duck to Poso.

In Swahili, as we saw in the sentences of (4) above, subject-verb inversion is also obligatory when an object NP is relativized. The subject NP of the embedded relative clause must occur immediately after its verb as in (4a); it cannot precede its verb or occur elsewhere after it as the ungrammaticality of (4c,d) attests. The main difference between the Dzamba-type languages and the Swahili-type is that in the latter, but not in the former set of languages, object relativization requires the retention of an anaphoric object clitic in the verb for the relativized NP. Further, with respect to agreement, subject relativization (cf. sentence (3)) as well as object relativization in the Swahili-type languages calls for complete subject-verb agreement in a manner characteristic of the pattern obtaining in simple sentences. We have seen above that this is not the case in the Dzamba-type languages: in these languages this type of agreement pattern is possible only between the verb of the main clause and its subject NP, that of the embedded clause agreeing with the head of the relative clause.

2.2.2 Object Relativization with Compound Verbs and Elsewhere. The agreement pattern and the phenomenon of subject-verb inversion discussed above are not restricted to constructions involving only a single verb: they occur also in constructions with double or compound verbs in all the languages under consideration here as may be seen from the Dzamba sentences (17) and (18) below:

(17) a. oPoso a-ba-áki o-lo-maa e-tobo waabo.
Poso she-be-Impf. Comp-to-sew a dress here
(Poso was sewing a dress here.)

b. e-e-tobo e-zik-í.
the-dress it-burned
(The dress is burned.)

c. e-e-tobo é-e-ba-áki oPoso o-lo-maa waabo e-zik-í.
RM-Ag-was Poso sewing
(The dress that Poso was sewing here is burned.)

d. *e-e-tobo oPoso a-ba-áki/ é-e-ba-áki o-lo-maa waabo e-ziki.
the-dress Poso she-was/ RM-Ag-was sewing here is-burned

(18) a. i-bi-bonga bi-ba-áki bi-kpe.
the-stools they-were Ag-short
(The stools were short.)

b. oMusa a-konz-áki o-lo-kela i-bi-bonga loome.
(Musa began to make the stools today.)

c. i-bi-bonga i-bi-konz-áki oMusa o-lo-kela loome bi-ba-áki
the-stools that-Ag-began Musa Comp-making
bi-kpe.
(The stools that Musa began making today were short.)

d. *i-bi-bonga í-bi-konz-áki o-lo-kela oMusa loome bi-ba-áki bi-kpe.

e. *i-bi-bonga oMusa a-konz-áki/ í-bi-konz-áki o-lo-kela loome bi-ba-áki bi-kpe.

That is, when an object NP is relativized from a structure involving compound verbs, the embedded subject NP must always immediately follow the first verb and the embedded verb must agree in number, person, and class-gender with the head of the relative clause; not with its logical subject NP. Sentences (17d) and (18d,e) are ungrammatical because they violate these restrictions. These Dzamba facts are paralleled in Lingala and Likila. Similarly, when an object NP is relativized out of a structure involving compound verbs in Swahili, the embedded subject NP must occur immediately after the first verb, as may be seen in (19) below:

(19) a. vi-tabu vi-me-potea tena.
books they-Perf-lose again
(The books are lost again.)

b. m-toto a-li-taka ku-nunua vi-tabu.
child he-ed-want to-buy books
(The child wanted to buy the books.)

c. vi-tabu a-li-vyo-vi-taka m-toto ku-nunua vi-me-potea tena.
he-ed-RM-them-want child to-buy they-lost
(The books that the child wanted to buy are lost again.)

d. *vi-tabu m-toto a-li-vyo-vi-taka ku-nunua vi-me-potea tena.

e. *vi-tabu a-li-vyo-vi-taka ku-nunua m-toto vi-me-potea tena.

Sentences (19d) is ill-formed because the embedded subject NP, m-toto, precedes its verb, and (19e) is ill-formed because m-toto occurs after the second verb rather than after the first.

It should be pointed out here that the phenomenon of subject-verb inversion in these languages is more pervasive than the discussion here may suggests. This phenomenon occurs obligatorily in the relativization of any VP-dominated (if there is such a node) constituent such as locative adverbs, instrumentals, and other relativizable elements (see Bokamba, 1975b for more discussion). The generalization here is that subject relativization, as we have seen, involves no subject-verb inversion; but the relativization of VP-constituents does. Also, contrary to Ross's (1967) Complex NP Constraint whereby elements contained in a sentence dominated by a NP with lexical head noun are prohibited from being moved out of that NP by transformations, relativization out of embedded clause is possible in all the languages under consideration here. The following Dzamba sentences illustrate this point:

(20) a. oPoso a-imol-áki oMusa kibo oJuma a-eza-áki ba-bana buku.
Poso she-tell-ed Musa that Juma he-give-ed the-children a book
(Poso told Musa that Juma gave the children a book.)

b. ba-bana bá:-imol-áki oP. oMusa kibo oJuma a-ba-eza-áki
the-children RM-told P. M. that J. he-them-gave

buku ba-kei.
book they-left
*(The children to whom Poso told Musa that Juma gave a book left.)

c. i-buku í-bu-imol-áki oP. oMusa kibo oJuma a-ve-eza-áki
the-book RM-Ag-told Poso Musa he-it-gave

ba-bana bu-kayei.[2]
the-children it-torn
*(The book that Poso told Musa that Juma gave the children is torn.)

d. *ba-bana oPoso a-imol-áki/bá:-imol-áki oM. kibo oJuma
Poso she-told/ RM-told Musa that Juma

a-ba-eza-áki buku ba-kei.

e. *i-buku oPoso a-imol-áki/í-bu-imol-aki oM. kibo oJuma
the-book Poso she-told/ RM-Ag-told

a-ve-eza-áki ba-bana bu-kayei.

f. *ba-bana bá:-imol-áki oP. oM. kibo oJuma a-eza-áki buku bakei.
that Juma he-gave a book

g. *i-buku í-bu-imol-aki oP. oM. kibo oJ. a-eza-áki ba-bana ...
that Juma he-gave

h. *ba-bana bá:-imol-áki oP. oM. kibo bá:-eza-áki oJ. buku bakei.
the-children RM-told Poso Musa that RM-gave Juma book ...

i. i-buku í-bu-imol-áki oP. oM. kibo í-ve-eza-áki oJ. ba-bana ...
the-book RM-Ag-told Poso Musa that RM-Ag-gave Juma the children

Sentences such as the Dzamba (20b, c) are not possible in English and many other natural languages, as the ungrammaticality of the English glosses attests. In Dzamba and many other Bantu languages such sentences are perfectly acceptable. What is interesting about relativization out of embedded clauses in these languages is that (1) subject-verb inversion occurs obligatorily as we expect; (2) subject-verb inversion occurs only in one of the clause, viz. the second embedded S; and (3) an anaphoric object clitic must be retained in the verb of the clause from which the NP is relativized. Sentences (20d, e) are ill-formed because subject-verb inversion has not occurred, and thus violate the first restriction. Sentences (20f, g) are ungrammatical because they violate the third restriction, i.e. there are no anaphoric object clitics retained in the verbs of the embedded relative clauses from which the NP's are relativized. Finally, sentences (20h, i) are ungrammatical because subject-verb inversion has occurred in both embedded relative clauses.

3.0 Motivation for Subject-Verb Inversion

Given all the facts that we have considered thus far, there are two major questions that come to mind: (1) Why does the verb of the embedded relative clause in Dzamba and related Bantu languages, but not in the Swahili-type, agree with the head of the relative clause rather than its deep subject noun phrase? And (2) why is there a Subject-Verb Inversion in the embedded relative clause in object relativization in the Dzamba-type and Swahili-type languages?

Givón (1972b), working with data provided in Bokamba (1971), Meeussen (1972), and Takizala (1972), among others, attempts to answer these questions by proposing what he calls a Universal Pronoun Attraction Principle. This principle states that:

(21) Relative pronouns tend to appear directly adjacent to the head noun modified by the relative clause (Givón, 1972b: 190).

Givón (1972b) observes that when a subject noun phrase is relativized in the Bantu languages considered here, there is no subject postposing (our Subject-Verb Inversion) because the relative pronoun(sic) occurs adjacently to its head noun phrase. But when an object noun phrase is relativized, Givón (1972b) continues, this is not the case because the embedded subject noun phrase, as we have seen, occurs between the head noun modified by the relative clause and the relative pronoun--the languages being SVO. This situation, Givón (1972b: 192) maintains, clearly violates the universal pronoun attraction principle (21); thus to rescue it, the subject of the embedded relative clause must be postposed.

Givón (1972b: 192) suggests, in addition, that Subject-Verb Inversion (subject postposing in his terminology) in object relativization in Dzamba, Kihung'an, Swahili, and related languages is obligatory only when the relative pronoun is bound onto the verb. Givón (1972b) does not deal directly with the question of the contrast in subject-verb agreement in Dzamba-type and Swahili-type languages. He dismisses the Dzamba agreement pattern in relativization as a language specific morphemic constraint (Givón, 1972b: 193-94), and claims that agreement and subject postposing in theselanguages are unrelated problems. While this may be true of the Swahili-type languages, it is not true of the Dzamba-type. I return to this point below. Givón's (1972b) analysis is basically correct, but if it is inadequate in many respects. To see this, consider a second type of relative clause construction in Swahili which I shall refer to as the "amba-relative clauses" and a similar construction in Lingala, the "Eye-relative clauses." Let us examine the Swahili case first.

3.1 The "Amba"-Relative Clauses. Givón's (1972b) hypothesis that Subject-Verb Inversion in object relativization is obligatory only when the relative marker is bound onto the verb, but not when it is free or unbound, finds partial substantion in facts such as the following Swahili examples:

(22) a. m-toto a-li-ki-ona ki-tabu.
child Ag-Pst-it-see book
(A/the child saw the book.)

b. ki-tabu a-li-cho-ki-ona m-toto ni changu.
book Ag-Pst-RM-it-see child is mine
(The book that the child saw is mine.)

c. *ki-tabu m-toto a-li-cho-ki-ona ni changu.
book child Ag-Pst-RM-it-see is mine

(23) a. ki-tabu amba-cho m-toto a-li-ki-ona ni changu.
book say-RM child Ag-Pst-it-see is mine
(The book that the child saw is mine.)

b. ki-tabu amba-cho- a-li-ki-ona m-toto ni changu.
book say-RM Ag-Pst-it-see child is mine
(The book that the child saw is mine.)

That is, while in the set of sentences of (22) where the relative marker is bound onto the verb the subject of the embedded clause must follow its verb as the ungrammaticality of (22c) attests, in the pair of sentences in (23) where the relative marker is suffixed onto another element the subject may precede or follow its verb. In (23), but not in (22), Subject-Verb Inversion is optional; and the only apparent reason is that in the latter set the relative marker is bound onto the verb; whereas in the former set it is bound on a dummy element which Givón (1972b) considers a free relative pronoun.

3.2 The "Eye"-Relative Clauses. Facts such as those illustrated in (22) and (23) also occur in Lingala, a Dzamba-type language, that has a dialect in which demonstrative pronouns function as unbound or free relative pronouns. Dzamba, Likila, and Lomongo which have similar demonstrative pronouns do not use them in this manner. The Standard Lingala examples are as follows:

(24) a. Poso a-somb-áká buku lobi.
(Poso bought a book yesterday.)

b. buku bu-zal-áká ntalo.
book Ag-be-Pst. expensive
(The book was expensive.)

c. buku (é)-bu-somb-áká Poso lobi bu-zal-áká ntalo.
(RM)-Ag-bought Poso it-was
(The book that Poso bought yesterday was expensive.)

d. *buku Poso (é)-bu-somb-áká lobi bu-zal-áká ntalo.
book Poso (RM)-Ag-bought

e. *buku (é)-bu-somb-áká lobi Poso bu-zal-áká ntalo.
(RM)-Ag-bought Poso

(25) a. buku bu-ye Poso a-somb-áká lobi bu-zal-áká ntalo.
Ag-RM Poso she-bought yesterday
(The book that Poso bought yesterday was expensive.)

b. buku bu-ye bu-somb-áká Poso lobi bu-zal-áká ntalo.
Rel it-bought Poso yest. it-was
(The book that Poso bought yesterday was expensive.)

c. ?*buku Poso a-somb-áká lobi bu-zal-áká ntalo.
(The book Poso bought yesterday was expensive.)

d. buku bu-somb-áká Poso lobi bu-zal-áká ntalo.
Ag-bought Poso
(The book Poso bought yesterday was expensive.)

e. *buku bu-ye é-bu-somb-áká Poso lobi bu-zal-áká ntalo.
Rel RM-it-bought

The Lingala sentences in (24) illustrate the same point as the Swahili (22), and that is, when the relative marker is bound on the verb Subject-Verb Inversion is obligatory in the embedded relative clause if the relativized constituent is an object noun phrase. The same phenomenon will occur if any other VP-dominated constituent is relativized. Sentence (24d) is ungrammatical because the embedded subject noun phrase precedes its verb; and (24e) is ungrammatical because the embedded subject does not occur immediately after its verb.

The next set of sentences (25) shows that Subject-Verb Inversion is optional when the relative pronoun is unbounded; thus the embedded subject noun phrase may precede its verb as in (25a) or it may follow as in (25b). When the subject follows its verb, it is possible to omit the relative pronoun (apparently via a rule of relative clause reduction similar to English) as in (25d); but when the subject precedes its verb, it is not permissible to omit the relative pronoun as the ungrammaticality of (25c) attests. (25e) is ungrammatical because the relative pronoun cannot co-occur with the bound relative marker.

As I observed earlier, Givón (1972b) analysis and his universal pronoun attraction correctly predict these facts of Swahili and Lingala. The question that Givón's analysis does not attempt to answer, however, is why sentences such as the Swahili (23b) and the Lingala (25b,d), where the embedded subject noun phrases are postposed, to use Givón's terminology, occur at all. Put differently, given the fact that the relative "pronouns" amba-cho and bu-ye occur immediately after their respective head noun phrases as the pronoun attraction principle (21) predicts, why should the embedded subject noun phrase move at all?

Givón (1972b) may not have been concerned with this question at the time, but if his analysis is to be maintained, the question must be answered. If subject-verb inversion was solely necessitated by the Universal Pronoun Attraction Principle (21) as Givón (1972b) suggests, sentences such as (23b) and (25b,d) would not, it appears to me, occur. Further, when facts such as those Topicalization in the Dzamba-type languages are considered together with those of relativization out of the VP, Givón's (1972b) pronoun attraction principle becomes too weak.

3.3 A Conspiracy of Principles in Bantu. My contention is that a set of principles is involved in the derivation of the relative clause constructions considered here. These principles include (1) subjec-verb agreement, (2) a Double-Noun Phrase Constraint (see below), and (3) the Universal Pronoun Attraction Principle. These three principles, or any two of them, act together in conspiracy to maintain the cannonical word order of the Bantu languages under consideration here, viz. SVO. Let us consider how this hypothesis applies to the facts of relativization in the Dzamba-type and Swahili-type Bantu languages.

I observed earlier that in the Dzamba-type languages, as in most Bantu languages, every finite verb must agree in number, person, and class-gender with its subject noun phrase. This is a fact of Bantu languages that has been noted for a long time. In our analysis of relativization in the Dzamba-type languages, we saw that finite verb of the embedded clause, just like that of the main clause, invariably agreed with the head noun, which may be the object or the subject of the main clause. This type of agreement pattern, I observed, was very peculiar in view of the agreement rule stated in (5) which requires agreement between the deep subject noun phrase and its verb. Without completing the characterization of subject-verb agreement in Dzamba and related languages, I proceeded on with the presentation of additional facts on relativization.

Having thus presented all the relevant facts about relativization in both types of languages, I can now propose a hypothesis about the operation of subject-verb agreement in the Dzamba-type languages in contrast to the Swahili-type. My hypothesis is that subject-verb agreement in Dzamba and related languages operates on superficial, rather than deep, grammatical relations. Often this pattern of agreement amounts to having the verb agree with the noun phrase that immediately precedes it. I have shown elsewhere (cf. Bokamba, 1975b: chapter 3) that this is in fact the pattern of agreement found in common constructions such as Passive, Topicalization and certain conjoined coordinated structures where the subject NP's are all non-human.

The agreement pattern found in such constructions can be observed in the relative clauses such as the Dzamba (26c) below where the relativezed NP is embedded in an object NP, and where the verb of the embedded relative clause agrees with the NP immediately preceding it:

(26) a. oPoso a-en-éki i-bi-koti.
(Poso saw the hats.)

b. ba-bana ba-tom-el-áki oJuma i-bi-koti.
(The children sent the hats to/for Juma.)

c. oPoso a-en-éki i-bi-koti í-bi-tom-el-áki ba-bana oJuma.
Poso saw the-hats RM-Ag-sent-to/for
(Poso saw the hats which the children sent to/for Juma.)

d. *oPoso a-en-éki i-bi-koti ó-tom-el-áki ba-bana oJuma.

That is, the verb of the embedded relative clause must agree with the head NP of the relative clause (26c), which in this case is an object NP.

Sentence (26c) brings us to the second and third arguments for the hypothesis that a set of principles conspires in relativization in the Bantu languages under study here to maintain the basic SVO word order. In all these languages, as is the case in most natural languages, the relative clause modifies the head noun phrase; i.e. it functions adjectively with respect to the head noun. In most Bantu languages all adjectives follow their head NP's, generally immediately. In the relative clause the only element that signals any relationship to the main clause is the relative pronoun.

However, if subject-verb agreement in the Dzamba-type languages applies on superficial grammatical relations as I have argued, the relativization of an object NP from the structure underlying (26c), namely (27),

(27) $_S$(oPoso -en-éki ibi-koti $_S$(ba-bana -tomel-áki oJuma ibi-koti))

will surely violate these principles in that the embedded subject NP will intervene between the head NP and the verb containing the relative marker. Thus to maintain these principles of the Bantu grammar, the subject NP must be postposed, or better, the finite verb and the embedded subject must be inverted. Further, in sentences such as (23) and (25) where a free relative pronoun occurs, subject-verb inversion still applies, even though optionally, suggesting that more than word order is involved here. I suspect that the occurrence of this phenomenon in the "amba-" and "eye-relative clauses" is also necessitated by a surface constraint which prohibits the occurrence of structures such as *NP NP V X. That is, there is a language-specific surface constraint, which I shall call the Double NP Constraint, that exists in these languages. This constraint may be stated as in:

(28) <u>The Double NP Constraint</u>

> No two non-conjoined and non-correferential NP's may occur before a finite verb in any simple sentence or one that is intonationally defined as such unless the first NP is immediately followed by a relative pronoun.

What I am claiming here is that topicalized and relative clause constructions, but not dislocated constructions, are considered phonologically as having a single sentence intonational contour. This is a fact of the languages under consideration here. This constraint, in addition to the revised version of Givón's (1972b) Universal Pronoun Attraction Principle given below, will account for all the facts of relativization discussed here.

4.0 Conclusions and Implications

In the preceding sections I have discussed the process of relative clause formation in two Bantu languages, namely Dzamba and Swahili, whose facts are paralleled in three or four other Bantu languages. I have shown that subject relativization in these languages is basically similar, except in terms of subject-verb agreement. In the Dzamba-type languages

there is no agreement between the embedded subject NP and its verb, agreement being instead with the head NP of the relative clause. In Swahili and related languages, in contrast, the verb of the embedded clause agrees with its logical subject NP. I have shown further that object relativization in all the Bantu languages considered here characteristically involves subject-verb inversion in the embedded relative clause, and have argued that Givón's (1972b) Universal Pronoun Attraction Principle alone is not sufficient to account for the facts of relativization in these languages. I have suggested that a set of principles conspires to achieve the structures found in relative clauses in these Bantu languages.

If this analysis is correct, what it implies is that the rule of Subject-Verb Agreement stated in (5) above, and Givón's pronoun attraction principle (21) will have to be revised as in (29) and (30), respectively:

(29) Subject-Verb Agreement (Obligatory)

$_Z$[NP	#	$_S$[NP	V	X]	#	Y]	
1	2	3	4	5	6	7	oblig ==>
		[α class, β pers]					
1	2	3	4	5	6	7	
		[α class, β pers]	[α class, β pers]				

Conditions: 1) Either term 1 or 3 may be equal to zero, but not both;

2) When 3 is equal to zero, the verb of the embedded clause agrees with the NP immediately preceding it.

(30) Revised Universal Pronoun Attraction Principle

Relative pronouns or elements bearing relative markers will always occur directly adjacent to the head NP modified by the relative clause.

While rule (29) is admittedly unusual, it does account for the facts presented here. An alternative to (29) would be to extrinsically order the rules of Relative Clause Formation (6) and Agreement (5) one way to derive some of the sentences, and then reverse the order to derive the other sentences. This alternative appears to be very unsatisfactory in principle and less attractive than the revised rule above. Note also that the revised pronoun attraction principle, unlike the original, takes into account those constructions that involve bound relative markers.

Finally, unless one wishes to build-in the Double NP Constraint into the rule of Relative Clause Formation and thus possibly distinguish subject relativization from object relativization in the Bantu languages studied here, I believe that Ross's (1967) formulation of this rule (6) can be maintained under the analysis proposed here. Further, the analy-

sis of relative clauses in which the embedded subject NP immediately follows its verb as an instance of subject-verb inversion, rather than either subject postposing or verb-preposing, obviate the need for justifying the resultant word order. That is, if one assumes that this phenomenon involves the movement of either of the two constituents, one must present convincing arguments for the specific or fixed order in which the verb and the embedded subject NP occur in constructions where VP-dominated constituents are relativized. At the present time I know of no such arguments.

Footnotes

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[1]Common nouns in all Bantu languages consist of two morphemes: a noun prefix and a noun stem. In some Bantu languages, for instance, Dzamba, Likila, Luganda, among others, there is a third morpheme called the noun pre-prefix or vowel-initial that occurs in specified semantic/syntactic contexts. The occurrence of these pre-prefixes on a noun or set of nouns makes such nouns definite, in contrast to their non-occurrence which makes them indefinite. In Dzamba and Likila, the occurrence of the noun pre-prefixes is obligatory in the subject position, but optional in the object. Further, in Dzamba, but not in Likila, proper nouns also take these pre-prefixes.

[2]It should be pointed out here that except for nouns referring to humans, the subject prefixes and the anaphoric object clitics for nouns of other classes are phonologically identical in Dzamba and most other Bantu languages. But in certain cases, for instance in the Dzamba sentence (20c), phonological changes affect these morphemes in way that mask their similarities. This is the case for the morpheme -bu- and -ve- in (20c), the latter being derived from *-bu-e- via *-bw-e-.

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A MORE THAN PASSING LOOK AT PASSIVES: SWAHILI

Carl W. Johnson, Jr.
Michigan State University

Gone are the days when active and passive sentences were thought to have a simple relationship to one another, when a passive sentence could be derived from its corresponding active one by a simple transformation. In the areas of English composition and second language learning, simple operations on surface structures have some utility. It is useful to be able to transform an active construction into the corresponding passive one, or vice versa, when editing or rewriting an essay, story, or learned dissertation. When learning a foreign language, a student must usually learn to handle these types of relationships, and specifically on a surface-structure level. However, setting up relationships between surface structures has its hazards, certainly for the student of a foreign language, but even more so for the linguist. In this paper we will look at some passives and related structures in Swahili, as an example.

Bantu languages, with Swahili being probably the best-known among them, have a rich morphology, and most linguistics students have come up against the extensive system of nominal concord prefixes that characterize Bantu. What is perhaps less well known, but more interesting, at least to me, is the system of suffixes occurring throughout the verbal system. We are not interested here in Swahili morphology as such, but rather in the fact that Swahili uses its morphology, and specifically the suffixes affixed to a verbal root, to express many of the sorts of relationships between actions and the participants in those actions, while many other languages, such as English, express these relationships by means of prepositions, auxiliaries, and/or surface ordering of elements. As as example, Swahili passive verb forms always show the passive suffix -w. Another suffix is used to show that there is a beneficiary of the action, somewhat akin to the English use of an 'indirect object', but not the same.

A brief look at the structure of normal Swahili finite verb forms is given in Table I, on the following page. We have a system of prefixes, of which those listed under Subj and Obj must agree with the grammatical subject and object of the verb, respectively. The Tense/Aspect marker shown is Past. The Final Vowel need not concern us here, but the position labeled Extension Suffixes is where our interest really lies. Note that this is the only position that can be occupied by more than one morpheme in a given verb form.

TABLE I

Subj/	Tense/aspect/	Obj/	Vb root/	Extension suffixes/		Final Vowel	
a-	li-	zi-	-andik-			-a	aliziandika 'he wrote them (letters)'
a-	li-	wa-	-andik-	-i		-a	aliwaandikia 'he wrote to them (people)'
zi-	li-		-andik-		-w	-a	ziliandikwa 'they (letters) were written'
wa-	li-		-andik-	-i	-w	-a	waliandikiwa 'they were written to'

Let us examine some sentences.

(1) Mwalimu aliandika barua.
'The teacher wrote a letter (<u>or</u> some letters).'

(2) Mwalimu aliziandika barua.
'The teacher wrote the letters.'

(3) Mwalimu aliwaandikia wazazi barua.
'The teacher wrote letters to/for the parents.'

(1)-(3) are simple, straightforward, active sentences. (1) and (2) show the non-occurrence and occurrence, respectively, of what has been called the 'Object Prefix'. (3) has a beneficiary to the action and uses what has been called the Applied stem, indicated by the <u>-i</u> suffix, denoting that an action is being done to, for, or on behalf of someone. Early Swahili studies called this form the Prepositional, I prefer to call it the Benefactive. Note that the OBjPfx in (3) cannot refer to the <u>letters</u>, but must refer to the <u>receivers</u>. The terms 'direct object' and 'indirect object' are not very useful in Swahili, since these terms imply the interposition of a preposition, and the relationships involved are not what these terms imply. As necessary, we will distinguish two types of verb object: Object_1, which may appear within the verb form as an ObjPfx; and Object_2, which may not. These are categories of surface arrangements. Otherwise we will distinguish between them on the basis of their semantic, or rather Sememic, relationship to the action, using the terms Patient and Beneficiary. In sentence (3), <u>letters</u> is the Patient, and <u>parents</u> the Beneficiary.

If some or all of the noun referents in these sentences are omitted, we may have the following:

(4) Aliziandika. 'He wrote them (that is, the letters).'
(5) Aliwaandikia wazazi. 'He wrote to the parents.'
(6) Aliwaandikia barua. 'He wrote letters to them.'
(7) Aliwaandikia. 'He wrote to them.'

There is no way to refer to the Patient in sentences (5)-(7) by a choice of ObjPfx. If a Beneficiary is present, <u>it</u> must be referred to by the ObjPfx. Verb objects are not normally pronominalized by a free-standing pronoun, so here the only choices are omission or a demonstrative of some sort. In a broad sense there is no word for <u>it</u> in Swahili.

But we are concerned with passives. Let us look at the passive equivalents of these sentences.

(8) Barua ziliandikwa (na mwalimu).
'The letters were written (by the teacher).'
(9) Wazazi waliandikiwa barua (na mwalimu).
'The parents were written letters (by the T).'
(10) *Barua ziliandikiwa wazazi (na mwalimu).
'The letters were written to the parents (by T).'
(11) *Barua ziliwaandikiwa wazazi.
'The letters .....'

Sentence (8) gives us no problem. We move the 'direct object' to Subject position, use the Passive stem of the verb (with the suffix -w), and we can optionally include the Agent, introduced by na. But in (9) we find that parents is the subject, not letters, and as a matter of fact, (10), with letters as subject, is ungrammatical. This is not to say that the equivalent of the English gloss to (10) is not possible, but it is possible only by using some periphrastic construction to show the Benefactive relationship, not the natural way to express the idea, and in any case, not the passive of sentence (3). Sentence (11) is no improvement, since the ObjPfx is not permitted with the Passive stem. English allows some flexibility here where Swahili does not. With some English verbs, either the Patient or the Beneficiary may be realized as the subject of a passive sentence; the English glosses to both (9) and (10) are grammatical. But Swahili has a restriction such that if both Patient and Beneficiary are realized in the structure, only the Beneficiary can be the subject of the passive verb.

Sentences (12) through (16) provide some more examples.

(12) Mwalimu alivirudisha vitabu.
'The teacher returned the books.'
(13) Mwalimu aliwarudishia watoto vitabu.
'The teacher returned the books to the children.'
(14) Vitabu vilirudishwa (na mwalimu).
'The books were returned (by the teacher).'
(15) Watoto walirudishiwa vitabu (na mwalimu).
'The children were returned the books (by the T).'
(16) *Vitabu vilirudishiwa watoto (na mwalimu).
'The books were returned to the Ch (by the T).'

I find the English gloss given for (15) at best only marginally acceptable, and the real English equivalent for (15) is the gloss for (16). (16) could be grammatical only in the sense of: 'The children were given back to the books.'

We usually think that only transitive verbs can have passives. Obviously, if the object is going to become the subject, you have to have an object in the first place. In the broad sense, this is true in Swahili, of course, but the use of verbal extension suffixes upsets some of the usually expected relationships. In sentences (17)-(19) we have a verb of motion, return, go back, which can normally be followed by a locative expression, that is, a place, or by some sort of manner or time adverbial, but which cannot take an object. But the verb return also has a Benefactive stem, as in (18),

benefactives always have an object, and this can be expressed as a passive, (19). Although this last construction is not terribly common, it does occur, sometimes where least expected.

(17) Mwalimu alirudi shuleni.
'The teacher returned to the school.'

(18) Mwalimu aliwarudia watoto.
'The teacher returned to the children.'

(19) Watoto walirudiwa na mwalimu.
'The children were returned to by the teacher.'

Some of these sentences are similar to the Japanese affective passives mentioned by Noriko McCawley (McCawley, 1972). Note:

(20) Mtoto aliharibu tivii ya Hirosi.
'A child damaged Hirosi's TV.'

(21) Mtoto alimharibia Hirosi tivii.
'A child damaged for Hirosi his TV.'

(22) Hirosi aliharibiwa tivii na mtoto.
'Hirosi had his TV damaged by a child.'

Sentence (22) is a fairly close paraphrase of McCawley's sentence (13). An obvious difference is that in Swahili there is definitely an active counterpart to these passives. There are, however, a few Benefactive Passives in Swahili for which the active counterparts would not normally be used, although they are possible.

(23) Waliishiwa na chakula. (-isha = 'to be done, gone')
'They were out of food (They were finished-to by food.)

(24) ?Chakula kiliwaishia. 'The food is gone for them.'

(25) Watoto walifiwa na baba. (-fa = 'die' -fiwa is Benef.Pass.)
'They were bereaved of their father.'
(They were dies-to by father.)

(26) ?Baba aliwafia watoto. 'Father died on the children.'

These latter examples would be more commonly used with the Perfect T/A marker, rather than the Past which I have used here for the sake of limiting the number of different morphemes used, but in the right context the Past is normal.

In all the Swahili examples given so far, we have seen an S-V-O order, and an S-V-Ben-Pat order for those sentences including the Beneficiary. This order is normal, but not required. Although most examples might be regarded as somewhat archaic, an O-V-S order is possible, and an S-V-Pat-Ben order is also possible. These alternate orders may be considered 'marked' in some way, either as a means of emphasis, or because an element has a complex structure, rather than being a simple noun as in our examples. The point here is that the surface order, or possible surface order, is independent of any restrictions as to what may appear either as the subject of a passive sentence, or as the ObjPfx in an active one. In English, on the other hand, it seems that the possibility of an 'indirect object' appearing as the subject of a passive verb is tied to the possibilities of word order following that verb, and specifically whether the 'indirect object' can appear without its preposition and immediately following the active verb.

There are related matters which both need some attention, and which can also help to clarify the picture of the passive. In English we have constructions which have the form of the passive, but which have been seen to be somewhat different, in that they do have a normal active counterpart. Sometimes they have been called 'stative passives' or just 'statives'. Sinha (1973) among others discusses these, and while he points up some aspects of the problem, he seems to miss the real point. As an example, he cites his informants as unanimously choosing a passive as over against an active alternate (for one example his sentences (27a & b), while the point is that the active sentence, because of its different point of focus, is inappropriate in the context. But appropriateness in a context is precisely what the whole matter of passives is all about. But let us look at some more Swahili data.

(27) Mwalimu alifunga mlango. 'The teacher closed the door.'
(28) Mwalimu aliwafungia watoto mlango.
'The T closed the door for (or on) the children.'
(29) Mlango ulifungwa (na mwalimu).
'The door was closed (by the teacher).'
(30) Watoto walifungiwa mlango (na mwalimu).
'The children had the door closed for them (by T).'
(31) *Mlango ulifungiwa watoto (na mwalimu).
'The door was closed for the children (by T).'
(32) Mlango ulifungika. 'The door was closed.'

Sentences (27)-(31) completely parallel (12)-(16), but here we have the addition of (32). If we omit reference to the Agent in (29), (29) and (32) may be glossed the same way. They do not mean the same thing, however. (29) is a true passive, in which the Patient is the subject of the sentence, but the Agent, though not obligatory, <u>may</u> be expressed. Lyons makes the point that most passives are used without the agent being expressed, and in fact this is why they are used in the first place. This is true, but we may also say, certainly for Swahili, that the passive construction (or the passive suffix) carries with it the implication of Agent. Sentence (32) however, does not carry this implication, in fact it specifically excludes the possibility of expression of Agent. It is like the 'stative passives' in English which express a condition or resultant state, rather than an action. (29) means that at some time in the past someone closed the door, and the passive construction allows us to focus on the door rather than on the someone, or perhaps we might say to de-focus the someone so that it subordinated or omitted altogether. (32), on the other hand, simply means that the door is in a closed condition. This and related forms have traditionally been called Stative or Neuter. Of the possible participants in an action that we have discussed, the Neuter can express only the Patient. Rather it would be more proper to say that the Neuter realizes the situation in which only the Patient is present. In English such forms can get thoroughly mixed in with passives, although the operation of tense and aspect can help to separate them, but in Swahili they are kept separate and distinct by the morphology. Swahili neuter forms may express condition, capability, or possibility, usually clarified by the Tense/Asp

marker. They all express an exclusive relationship between Action and Patient only.

Neuters can be paired with passives, or compared, as we have just done, but any relationship to active sentences is a very shallow one, at best. While both active and passive sentences express an event, the neuter does not. It expresses a condition or state preceding or following an event or potential event. Its relationship to actives and passives, then, lies not at all in the surface grammatical structure, but in the logical or event-line structure.

The structure of Swahili actives, passives and neuters is best seen as a function of the participants in an Action, such as Agent, Beneficiary and Patient, at a rather deeper level that the syntactic arrangements of surface sentences. In a Stratificational model, this puts it squarely into the Semology. The participants occurring in the semological structure, and the relationships between them, will determine the presence or absence of such morphemes as the Benefactive suffix, and will also determine the particular passive or other structure appropriate to the case. A predicate relationship involving Action, Agent and Patient, and optionally including a Beneficiary, will normally be realized as an active sentence. If, for whatever reason, Focus is placed on Patient or Beneficiary, a passive sentence will result. If the relationship involves only the Action and Patient, it will be realized by a Neuter.

Here we are considering Focus quite narrowly, as the specification of what will be realized as Subject of the verb. The Agent is considered normally in Focus, giving us the unmarked case. Focus in Swahili is subject to a restriction, namely, that it cannot be on Patient if Beneficiary is present. Note that this restriction is similar to one mentioned earlier, with respect to the ObjPfx.

I have hinted the Stratificational approach to analyzing the structure of active/passive/neuter sentences. There are other approaches, of which you are well aware. The basic premise of Generative Semantics, that the whole business must be derived from semantic structure, I find wholly agreeable. However, the increasing abstractness and gradual disappearance of deep structure, leading to a merger of semantics and syntax, or at the least a blurring of distinction between them, is a genuine problem, and another is the expression of deeper grammatical or semantic relationships as 'higher verbs/pro-verbs/predicates/sentences'. I believe that Jackendoff's criticism of lexical items which never appear at the surface, but which are always deleted by transformations whose sole purpose is to delete them, is a valid one. Fillmore's Case Grammar, and Jackendoff's Thematic Relations are attempts to explicate deeper relationships with new terms or concepts, having a relationship to surface phenomena, but on a different level of abstraction than categories of surface relationship. The system of Thematic Relations seems to accomplish this more readily, particularly in allowing a surface

element to realize more than one underlying relationship, and in clearly preserving the distinction between syntax and semantics. Alas, the real role of semantics in Interpretivist theory is not nearly as clear as one would like, or rather, the apparent role of Semantics therein, subsidiary at best, is inadequate.

The separation of analysis into discrete levels allows the use of notions akin to Thematic Relations to describe underlying relationships, while maintaining the primacy of semantic input. The Stratificational model as developed so far, while far from complete in the area of Semology, nevertheless allows us to separate out the necessary functions and relationships to account for the surface data, and to provide some insight into the workings of language.

Taking the Swahili data presented in this paper, we may analyze the semological relationships (the Semotactics, if you will) as follows: The first consideration is whether a predication is neutral or not. If we need a label, we might call the non-neutral one agentive, but labels are not vital. If it is neutral, the sentence will include only the verb, normally with a Neuter stem, and the Patient, as Subject. Subject, we note, is not a Sememic category, but a Lexemic one. A neutral predication also imposes severe restrictions on adverbial elements such as time as manner, which are beyond the scope of this paper. If the predication is not neutral, the next consideration is whether Focus is present. If so, the sentence will include the verb as a passive stem, and either the Beneficiary as Subject, with Patient optionally as $Object_2$, or the Patient as Subject, and with either choice the Agent may be optionally present introduced by <u>na</u>. If Focus is not present, the sentence will be the unmarked Active, with Agent as Subject, and Beneficiary and Patient present and accounted for as described earlier. In addition, the presence of the Sememe Beneficiary will normally be realized also by the Benefactive suffix. A diagram of this set of relationships in graphic notation appears as Fig. 1. A diagram of the lexotactics, giving the actual sentence strctures, appears as Fig. 2.

We have explored a bit of the semological structure of Swahili, with its semotactics and a portion of lexotactics. Swahili is perhaps as easy language to work with in this regard, because of its morphological richness, which readily separates various types of relationships at the surface. The actual situation is somewhat more complex than might at first appear, because we really do not expect that there will be a one-to-one correspondence between, say, the occurrence of particular verbal extension suffix morphemes and certain sememes such as S/Neutral/ or S/Focus/. Some simple verb stems will always realize a S/Beneficiary, some are always Neuter. But this lack of one-to-one correspondence is precisely the reason for separation into layers of structure, into strata.

Fig. 1 - A portion of Swahili Semotactics

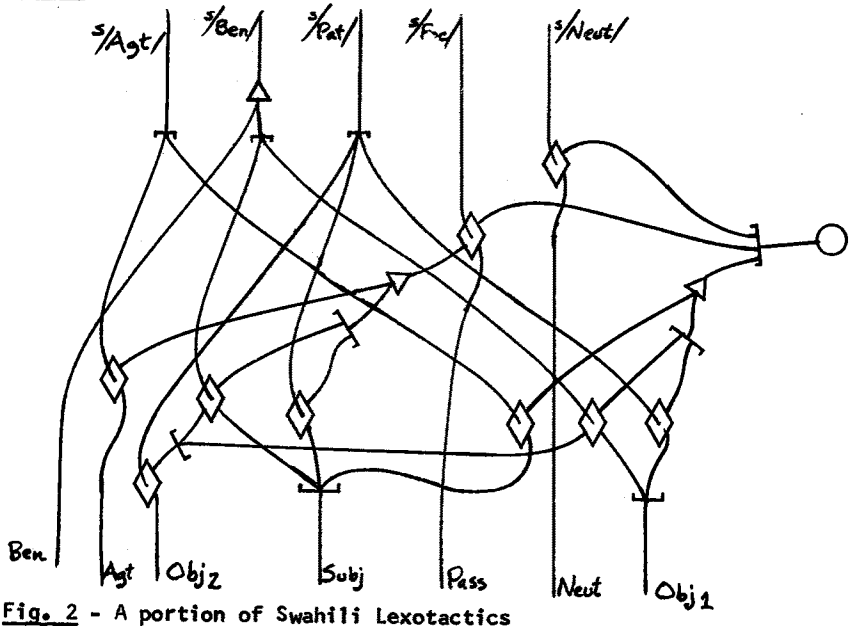
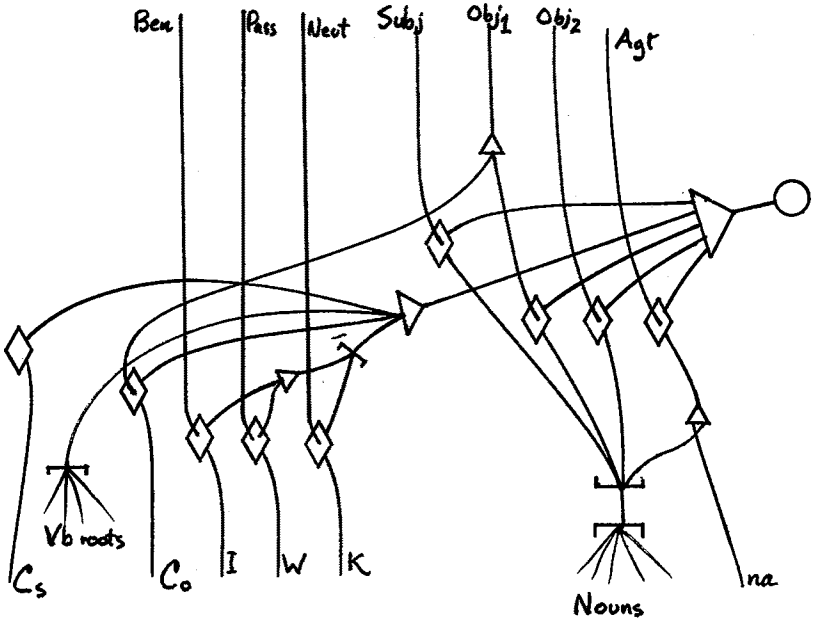


Fig. 2 - A portion of Swahili Lexotactics



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WHAT STRIKES ME ABOUT PSYCH-MOVEMENT*

Noriko A. McCawley
University of Chicago

I. Introduction: What is Postal's Psych-Movement?

Consider the following pair of S's.

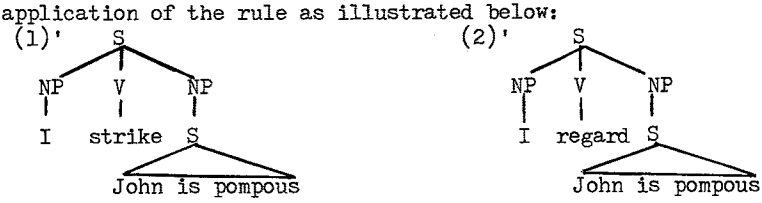
(1) John strikes me as being pompous.

(2) I regard John as being pompous.

The two Ss are nearly synonymous and yet the syntactic roles of John and I are exactly opposite; in (1) John is the subject, while it is the object in (2). Also, in (2) I is the subject, while it is the object in (1). The question to ask would be, then: is this just an isolated, idiosyncratic property of English verbs such as strike and regard? If the answer is yes, then we will treat this problem as purely a matter of lexicon as in the case of idioms. In Aspects, Chomsky took this position and maintained:

> The rules involving contextual features may be partially independent of semantic properties. Such examples must be borne in mind if any attempt is made to give some substance to the widely voiced (but, for the moment, totally empty) claim that semantic considerations somehow determine syntactic structure or distributional properties. (1965:229)

Postal (1970), on the other hand, took a diametrically opposed approach. He suggested that verbs such as strike and seem are essentially different from verbs like think and regard in that they share the semantic property something like "inherently subjective." And there is a syntactic rule, Psych-Movement, which is sensitive to this semantic property, obligatorily interchanging the underlying subject and object. According to this hypothesis, the underlying syntactic structures of (1) and (2) would look quite the same before the application of the rule as illustrated below:



The first thing I wish to show in this paper is that Postal's analysis is much closer to the truth than Chomsky's: namely, what is relevant here is the inherent semantic relationship between the human NP and the predicate. However, I claim that Postal's analysis is also far from adequate. Consider (3)-(9). If we read Postal (1970) carefully, we realize that he is claiming that the same rule is responsible for the derivations of the S's such as these:

(3) It strikes me that John is pompous.
(4) Shirley reminds me of my mother.
(5) This soup looks/tastes/smells funny (to me).
(cf. I looked at/tasted/smelled this soup.)
(6) My head aches/itches.
(7) Bill disgusts/frightens me.
(8) This house belongs to me.
(9) It is obvious/important/vital (to me) that Shirley loves Sam.

Clearly, his hypothesis of interchanging the underlying subject and object cannot explain several things we observe here. First, his rule cannot explain why we have me in (3), (4), and (7), while we have to me in (5), (8) and (9). Also, note that in (5) and (9), to me is deletable, while that is not the case with (8). Finally, why do we have my in (6) instead of me or to me? I now would like to suggest that a "common denominator" of S's pointed out by Postal is not the semantic property "inherently subjective" but rather something like "unself-controllable/unvolitional" on the part of the human NP involved in the situation. This would allow us to include S's such as the following which Postal failed to treat:

(10) The thought/idea occurred to me that ...
(11) It occurred to me that ...

The typical states of affairs in which humans are "unself-controllably" involved would be as follows:

(12) i) non-intentional sensory and mental experiences (through eyes, ears, tongue, skin and the '6th' sense)
ii) emotional experiences
iii) physical and biological experiences such as hunger/cold/headache, etc.
iv) competence/potential
v) need
vi) (inalienable) possession
vii) 'happenstance' as expressed by verbs such as 'happen' 'occur' or 'befall'.

Notice that all of Postal's Psych-Movement S's, perhaps with the exception of (9), fit nicely into these categories, together with the additional (10)-(11) type S's.

II. Psych-Movement Like Phenomena

The second thing I want to show in this paper is that any adequate theory of natural languages must account for the Psych-Movement phenomenon in Modern English in relation to many seemingly unrelated phenomena in many languages of the world. More specifically, I mean phenomena such as the following:

(13) i) the "Dative subject" constructions in Modern Russian
ii) the "Dative subject" and the "marking of object by ga, otherwise the subject marking particle in Japanese"
iii) the "indirect verbs" in Georgian
iv) the special use of Dative, Locative and Possessive case in Avar
v) impersonal constructions such as in Old English, Modern German and Tamil
vi) impersonal existential constructions in Japanese

In this section, I will discuss (i)-(v). (vi) will be discussed in the next section.

Russian "Dative subject" constructions:

The so-called dative subject constructions in Russian include S's such as the following:

(14) i) Jemu (D) xolodno (3rd person Neuter).
lit. "to him it is cold" for "he feels cold"
cf. St'ena (Nom.) xolodna (3rd person Fem.).
"the wall is cold"
ii) Mn'e (D) bol'it golova (N).
lit. "to me hurts head" for "my head hurts/aches"
iii) Ja (N) xoču vyjt'i. "I want to go out"
Mn'e (D) xočetsa vyjt'i. "I want to go out" (Not very assertive; 'because I have to go the bathroom')
iv) Jemu (D) nrav'itsa eta kn'iga (N).
lit. "to him likes this book" for "he likes this book"
v) Mn'e (D) pr'ijatno zd'es'.
"to me it is pleasant here"
vi) Ovošči (N) mn'e (D) vkusny.
"vegetables to me are tasty"
vii) Jej (D) nužna praktika (N).
"to her is necessary practice"
viii) Jej (D) n'eobxod'ima praktika (N)
"to her is essential practice"
ix) On (N) n'e sp'it. "He isn't sleeping"
Jemu (D) n'e sp'itsa. lit. "to him it doesn't sleep" for "he can't sleep"

In English, S's such as "he is cold" is ambiguous; (1) he feels cold and (2) he is a cold-hearted person. However, in Russian, such an ambiguity does not exist. In the first reading, he is in the dative case, while in the second reading he is in the nominative case, as illustrated by the contrast in example (i). Also, please note that in the first reading, there is no subject-verb agreement. The adjective is 3rd person neuter. In the second reading, on the other hand, the adjective is in the 3rd person masculine, agreeing with nominative case NP. Thus, in Russian dative subject constructions, there is 'agreement' between the nominative NP and the predicate. Consider (iv), corresponding to English "he likes this book". Book is in the nominative case and the verb like agrees with it. This is highly interesting, since it is different from Old English impersonal constructions as we will see later.

Japanese "Dative subject" constructions:

Kuno (1973:79-95) discusses the following unusual case-marking phenomenon: Japanese is a nominative-accusative language just like familiar Indo-European languages. Normally, the subject is marked by the nominative marking particle ga and the direct object by the accusative marking particle o. Thus, the NP_1-ga (subject) NP_2-o (object) Verbal" pattern results:

(15) Ken-ga Naomi-o nagutta. "Ken hit Naomi"

However, there is a class of verbals which mark their direct object by ga. Thus, for those verbals, we have "NP_1-ga (subject) NP_2-ga (object) Verbal". In addition, there is a rule, ga-ni Conversion,

which optionally changes the subject "NP_1-ga" to "NP_1-ni", resulting in the pattern "NP_1-ni (subject) NP_2-ga (object) Verbal".[2] The particle ni is a Dative/Locative marker. So, what? you might ask. Indeed, the comparison of the relevant data with English and Japanese alone do not strike us as anything interesting. However, when we compare Japanese and Russian, we immediately realize that the relevant verbals in both languages denote situations in which the human experiencer is unvolitionally/unself-controllably involved. Consider:

(16) i) (*Boku-ga/ni/no) atama-ga itai. lit. "head is sore"
I N D Poss. head sore for "my head hurts"
ii) Kare ga/*ni hon ga suki na koto
he N D books N like that
"the fact that he likes books"
iii) Anokoro ga (boku ni-wa) natukasii.
those days N to me D long for
"I long for those days"
iv) Anata no kimoti ga (boku ni-wa) wakar
" your feeling N to me D understand
"I don't understand you."
v) Henna oto ga (boku ni-wa) kikoeta ga, (kimi-niwa)
strange noise N to me D audible but to you D
Kikoenakatta ka?
Neg. Q
"I heard a strange noise but didn't you hear that?"
vi) Kimi niwa okusan ga hituyoo da.
to you D wife N necessary is
" You need a wife"
vii) Sono ongaku ga (boku ni-wa) kokoroyokatta.
the music N to me D pleasant was
"the music was pleasant to me"

What is so interesting is that this intriguing phenomenon is not limited to the nominative-accusative languages. Georgian, a partially nominative-accusative and partially ergative-absolutive language of the East Caucasus[3] and Avar, a trully ergative-absolutive language of the East Caucasus also exhibit basically the same phenomenon.[4]

Georgian "indirect verbs" and German impersonal constructions: Georgian has a large class of verbs called "indirect verbs." Tschenkéli (1958:449) remarks:

> Die Indirekten Verben bezeichnen, wenn auch night immer, so doch hauptsächlich Gefühle, Empfindungen, Bedürfnisse, Gemüts- und Seelenzustände, wie z.B. Lieben, hassen, hungrig sein, durstig sein, brauchen, wünschen, sich freuen, glauben usw. Dementsprechend werden diese Verben auch "Empfindungsverben" genannt.

Compare a pair of his examples.

(17) i) mama-s (Subj:Dat/Akk.) u-qvar-s svil-i. (dir. Obj:Nom.)
"Der Vater liebt das kind."
ii) student-i (Subj:Nom.) çer-s ceril-s. (dir. Obj:D/A)
"Der Student schriebt einen Brief."

It is highly noteworthy that Tschenkéli reminds the reader of the fact that Georgian indirect verbs and Modern German impersonal

verbs constitute a natural class, by citing the following examples.

(18) es gefällt mir für: ich finde Gefallen daran
es graut mir für: ich empfinde Grauen
es schwindelt mir für: ich empfinde Schwindel
es träumt mir für: ich träume
mich hungert für: ich habe Hunger, ich bin hungrig
mich friert für: ich friere
es freut mich für: ich freue mich

Avar:

In Avar, the mental verbs mark their subject with the dative marker, while the verbs of non-intentional perception mark their subject with the locative marker. I owe this information as well as the following examples to Anderson (1974).

(19) i) ins:u-c:a j-as j-ec:-ula
father-erg fem-girl f-praise-pres
"the father praises the daughter"
ii) ins:u-je j-as j-oχ'-ula
father-D fem-girl f-love-pres
"the father loves the daughter"
iii) ins:u-da j-as f-ix-ula
father-Loc f-girl f-see-pres
"the father sees the daughter"
iv) ins:-l j-as j-igo
father-gen f-girl f-is
"the father has a daughter"

Old English Impersonal Construction :

In his Progress in Language, Jespersen states that "A great many verbs which in Old English were impersonal have become personal in Modern English. (1894:216)" Consider some examples of OE impersonal verbs, taken from Gaaf's The Transition from the Impersonal to the Personal Construction in Middle English (1904). D and A stand for the Dative or Accusative case to which the human NP is assigned:

20) mætan, 'dream' (D or A)
þyncan, 'seem' (D)
lystan, 'please' (A)
langian, 'cause desire, longing' (A)
eglian, 'molest, trouble' (D)
hyngrian 'be hungry' (D or A)
þyrstan 'be thirsty' (D or A)
scamian 'be ashamed' (D or A)
smerten 'hurt' (often without D, as in 'my finger smarts')
myster 'be necessary' (D)
wanten 'lack, be deficient' (D)
lakken 'have not' (D)
happeh ' befall' (D)

OE impersonal constructions differ from OE personal constructions in that there is no subject-verb agreement, even where there is a nominative case present: the verb is always in the 3rd person singular form. Due to limitations of space, I will not go into a detailed discussion, but let me point out a couple of problems we face in describing/accounting for OE impersonal constructions: first, notice that OE impersonal verbs include intransitive verbs such as 'hungry,

thirsty, dream' etc. Thus, it is obvious that Postal's proposal of interchange of underlying subject and object cannot possibly be applicable here. Quite importantly, this is equally true of Russian dative constructions, Georgian indirect verbs, Modern German impersonal constructions, etc. Then , how can we assign non-nominative case to the human NP on a principled basis? Second, how can we prevent subject-verb agreement from making transitive verbs agree with nominative case NP's? Clearly, the rule must be sensitive to the fact that the nominative case in impersonal constructions is different from the nominative case in personal constructions. See Noriko McCawley (forthcoming) for further discussion of these points as well as of the relevance of the OE impersonal construction to Psych-Movement phenomenon in Modern English.

Now, let me conclude this section by the following remark: although Postal was extremely vague in specifying just how exactly his Psych-Movement rule operates, and there is also good reason to believe his particular analysis is inadequate, still he deserves due credit for attempting for the first time to formalize the great tendency in the human language as we have observed here of not assigning nominative/ergative case to the human experiencer who is unvolitionally/unself-controllably involved in the situation.

III. Have/Be Constructions and Psych-Movement Constructions: a Case Study of Impersonal Existential Constructions in Japanese

The third thing I want to show in this paper is that any adequate analysis of Psych-Movement phenomena should account for their close semantic relationship with Have and Be (and perhaps also feel and find) constructions. Let us look back at the two examples with which this paper has begun:

(1) John strikes me as being pompous.
(2) I regard John as being pompous.

It has been often pointed out that "such sentences as 'John strikes me pompous,' 'his remarks impress me as unintelligible' do not passivize, although the sentences 'I regard John as pompous,' 'it struck me blind,' and so on, are freely subject to passivization. (Chomsky 1965: 229)." Why don't they passivize at all? I would like to suggest that the answer must be sought along the same lines as the answer to the question of why Have and Be do not passivize. Consider our (12) again, which represents typical situations in which humans are unself-controllably/non-volitionally involved:

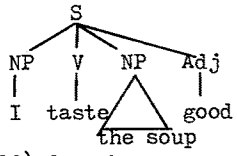
(12) i) non-intentional sensory and mental experiences (through eyes, ears, tongue, skin and the '6th' sense)
ii) emotional experiences
iii) physical and biological experiences such as hunger/cold/headache, etc.
iv) competence/potential
v) need
vi) (inalienable) possession
vii) 'happenstance' as expressed by verbs such as 'happen', 'occur' or 'befall'.

It is obvious that indeed many languages including English utiliz Have and Be for describing many, if not all, of the situations cited

above. Next, consider (20), for which Postal conjectures (21) to be the deep structure before the application of the Psych-Movement rule.

(20) The soup tastes good (to me).

(21)



Notice that (20) does have a paraphrasal relationship with (22) and (23).

(22) (To me) This soup has a good taste.

(23) There is a good taste to this soup.

I do not see how anybody's theory, either Postal's or Chomsky's, can predict this interesting fact.

In the following, I will briefly discuss Japanese counterparts of S's like (20), (22) and (23), which involve a special existential construction with an impersonal use of verb suru 'do'. This construction asserts the existence of invisible things such as noise, voice, smell, taste, touch, inner feeling/sensation, etc. which are sensed by the first person experiencer. An important characteristics of this construction is that the first person experiencer may not appear in the sentence. Consider:

(24) ears: Ame no oto ga (* boku-niwa) sita. "I heard the
rain 's sound N to me D did sound of rain"

eyes: *Henna yama ga (*boku niwa) sita.
strange N
mountain

cf. Henna yama ga (boku niwa) mieta.
visible

lit. "A strange mountain was visible (to me)
for "I saw a strange mountain"

nose: Ii nioi ga (*boku niwa) suru.
good smell N to me D

lit. "A good smell does" for "Something smells good"

tongue: Ii azi ga (*boku niwa) suru.
taste to me D do

lit. "A good taste does" for "This taste good"

touch: Kono sofaa wa (*boku niwa) yawarakai tezawari ga
this sofa Loc to me D sofe hand-touch N
suru.

lit. "Soft hand-touch does in this sofa" for "This sofa feels soft."

Mental: Henna yokan ga suru. "A strange presentiment does"
for "I have a strange presentiment"

Now, compare:

(25) i) Ame no oto ga (*boku niwa) suru.

ii) Ame no oto ga (boku niwa) kikoeru.
audible

It is extremely difficult to point out just what is the difference in meaning between the above pair. Both S's are correctly translated

into English as "I hear the sound of rain." Now, what is significant for the present discussion is that, first, this construction is also used for physical/biological experiences:

(26)i) (Boku wa/?* niwa) zituu ga sita.
headache N did
"Headache did" for "My head ached/I had a headache."
(cf. Boku wa z tuu/hakike o kanzita)
N Acc. felt/experienced

ii). (Boku wa/?* niwa) hakike ga sita.
desire for vomiting
"Desire for vomiting did" for "I wanted to vomit"

Note that this construction seems to allow the existence of Experiencer NP as the syntactic subject. Incidentally, I feel that this must be crucially related to the so-called 'double' subject in Japanese. Now, Second, the same impersonal use of <u>suru</u> 'do' shows up in the inalienable possession constructions:

(27) i) Naomi ga ii asi o site iru.
N good leg Acc doing is
"Naomi is doing good legs" for "Naomi has good legs"

ii) Ken ga ii koe o site iru.
voice
"Ken is doing a good voice" for "Ken has a good voice"

It seems that there is a good reason why the Japanese language had to develop such a special construction. First, the Japanese existential verb <u>aru</u> usually does not tolerate an invisible entity as its subject except when <u>aru</u> is used in the sense of 'take place'.

(28) i) Tukue no ue ni hon ga aru/*suru.
desk 's top at book N
"There is a book on the desk"

ii) Kinjyo ni ookaji ga atta/*sita.
neighborhood in big fire N
"There was a big fire in the neiborhood"

iii) Utijyuu bara no ii nioi ga sita/*atta.
all over house rose's good smell
"There was good smell of roses all over the house"

Second, the Japanese verb <u>motu</u> 'have' is different from English <u>have</u> in that (with some exceptions) it is normally used for alienable possession and in addition its subject is human:

(29) English: Naomi has good legs.
Japanese: Naomi ga ii asi o site iru/*motte iru.

(30) English: Ken has three copies of <u>Aspects</u>.
Japanese: Ken ga <u>Aspects</u> o sansatu motte iru/*site iru.
N Acc. three

(31) English: This desk has five legs.
Japanese: *Kono tukue wa asi o gohon motte iru/site iru.
(cf. Kono tukue wa/niwa asi ga gohon aru)
"There are five legs in this desk" for
"This desk has five legs"

FOOTNOTES

* Earlier version of this paper was read at the fifth Annual Meeting of New England Linguistic Society held at Harvard University, 1974. I am grateful to many people both at NELS meeting and at LACUS meeting who encouraged my work by useful comments. I also owe a special debt to Karen Richards who provided me with Russian examples and also called my attention to the Georgian facts.

1 I think our analysis will eventually lead to the systematic study of more idiomatic expressions such as:
(i) The thought crossed my mind that ...
(ii) It just hit me that ...

2 Each relevant predicate must be marked in the lexicon as to whether it is going to undergo this rule or not.

3 Georgian is a nominative-accusative language in the present tense, but it becomes an ergative-absolutive language in the aorist tense. For the revealing discussion of case systems in Georgian, see Aronson (1970).

4 For further discussion of the relevance of this fact to the linguistic theory, see Noriko McCawley (forthcoming).

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IV

SEMANTICS AND BEYOND

THE SEARCH FOR SEMANTIC UNITS

Allen Walker Read
Columbia University

Statements about language come in all degrees of technicality. At one end of the scale is the sentence that the journalist Max Lerner set down as the opening of one of his columns, when he said: *"A word is the skin of a thought."*[1] This could be the basis of a theory of linguistics; and then linguistics, to continue the figure of speech, would be a form of dermatology. Actually, the most popular view of language may well be just that: it is merely the skin of our thoughts. I do not agree with this view, but some well-considered treatments seem to me to share its shortcomings.

The jumping-off point of this paper was a phrase that I found in Wallace Chafe's book of 1970, *Meaning and the Structure of Language.* At several points he speaks of a "semantic inventory," which apparently refers to a listing of units in the world outside language that may correspond to units of form inside language. This is an untenable position if one makes the assumption, as I do, that the world outside language is a flowing process, without discrete units.

The assumption that the world is a continuous flow is an old one, going back to the view attributed to Heraclitus that one cannot step into the same river twice. In a practical way, most of us simply don't believe it, because, as I shall argue here, we are prisoners of our linguistic systems. We will also have to look into what the physicists may be able to tell us.

Chafe's position is carefully argued, and deserves respectful treatment, but I am puzzled to find out how he arrives at his "semantic inventory." The underpinning statement may well be this (p. 20): "the conceptual universe, and then necessarily also the universe of symbols, may be discretely structured." He finds it easy to develop the notion of "semantic units" because of the vocabulary that he allows himself. He assumes something countable when he speaks of "ideas" and "concepts." Typical sentences are these:

> The simplest conceivable communication system is one in which only a single idea can be transmitted. (p. 19.)
>
> Language enables a speaker to transform configurations of ideas into configurations of sounds. (p. 15.)
>
> [Language operates] by converting ideas into a medium which does have the capacity to pass between one nervous system and another. (p. 16.)
>
> The sound . . . is normally reconverted within their nervous systems into some facsimile of the original concepts. The facsimile is usually imperfect . . . between the conceptual repertoires of different individuals. (p. 17.)
>
> The more recent growth of the human conceptual inventory in large part has been a matter of adding concepts that are more abstract to a basic inventory of concrete concepts. (p. 48.)

The closest that Chafe comes to an explanation of these entities is as follows (p. 16): "Ideas, I assume, have some kind of electro-chemical existence in the nervous systems of individuals." This terminology begs so many questions that I believe he has not established any foundation for an inventory.[2]

I agree with Charles F. Hockett, who stated in one of his early papers (if 1948 is early) as follows: "Other social scientists use terms such as *idea, mind, concept* as common-vocabulary words; the linguist must not, for part of his task is to investigate the operational definition of these terms and attempt their translation into more fundamental behavioristic language."[3] I do not hold the view that dropping mentalistic terms like *idea, mind,* or *concept* would transform a linguist's writing into sound scientific discourse, for that would be word magic at its worst; but such terms do have an inevitable tendency to deflect or veer the discourse in certain directions.

Hockett's mention of "behavioristic language" raises the spectre of behaviorism, which means different things to different people. Nowadays most people think of B. F. Skinner, with his behavior modification, as representing behaviorism; but there was an older, more humane type, such as that promulgated in the 1920s by A. P. Weiss and embraced by Leonard Bloomfield.[4] It did not involve imposed behavioral modification; but modification might come from within by increased awareness of possible options.

Linguists in the Bloomfieldian tradition have been severely criticized for leaving meaning out of account; but this is a faulty accusation. They only refused to use meaning in certain traditional, circular ways. A meaning was too often reified, as in the phrase, "A word *has* a meaning," as if it were an entity. In the famous dispute in 1944 between Leo Spitzer and Bloomfield, Spitzer stated exactly that. As he wrote: "We mentalists . . . have no scruples in positing an entity 'anger' in observing its activity. But we could not expect a scholar with the scruples of Mr. Bloomfield to study the influence of 'anger' on speech—which is a reality: just consider the style, even the phonetic style, of Hitler."[5]

The Bloomfieldian point of view was that "meaning" was a suffusing element of all language by definition, to differentiate it from squeaks, thumps, creakings, etc. To use a figure of speech, the semantic continuum is a seamless fabric (although the threads of the fabric can be discerned and isolated). The often-quoted definition of language as given by Bloch and Trager in 1942 was as follows: "A language is a system of arbitrary vocal symbols by means of which a social group cooperates."[6] Although the word *meaning* does not occur there, the paraphrase "symbols by means of which a social group cooperates" takes its place. Leonard Bloomfield himself in 1943 wrote an essay entitled "Meaning," in which he asserted the need for a general term to cover the phenomena of cooperation achieved by language, and he concluded by saying (1943, p. 102): "The actual student of language, . . . if he does not want to coin a new term, . . . will naturally choose the traditional term meaning." In a study of my own in 1948, I cautioned that the ignoring of contexts would result in a "bloodless, nerveless, and meatless point of view" (Read 1948, p. 82).

What Bloomfield tried to avoid, as I said before, was the reification of meaning, which so usually has been couched in terms of *idea, thought,* and *concept.* Traditionally these represent a mentalistic domain, separate from the physical domain and hence a dualism has been forced on us. To avoid this dualism, a theory of abstraction has been developed, and it is crucial to understand that in the Bloomfieldian outlook an abstraction is not a mental manifestation. Features are selected from the environment not to make an entity, but to make a grouping that can be spoken of by the representation of a verbal symbol. Whether one can agree with or adopt the Bloomfieldian outlook depends altogether on whether one accepts the validity of a theory of abstraction.[7] Bloomfield introduced an abstract term when he wrote (1926, p. 155): "A minimum form is a *morpheme;* its meaning a *sememe.*" He was there naming his basic semantic unit, and it is closely tied to the form (the morpheme) and does not represent some outside entity. Could such sememes be inventoried? He answered (1933, p. 162) that "the sememes of a language—could be analyzed or systematically listed only by a well-nigh omniscient observer."

In contrast to this, Wallace Chafe's semantic units seem to make their appearance outside language and then find their symbolization when they are taken into the language

system. Such a view requires that the universe itself have natural segmentations, and this would contradict the Heraclitean assumptions. When I say that this is an assumption, I recognize that I am taking it as an act of faith. We might hope that reliable information might be found in the field of physics, but we are at the mercy of the particular physicists that we might consult. Historically many of them have attempted to find elementary segments that could be used as building blocks. Yet the soundest of them question the notion of fundamental particles. The discussion by David Bohm, Professor of Theoretical Physics at Birkbeck College, University of London, brings this out. In a paper of 1971 he said (pp. 13-14):

> One of the extreme forms of the fragmentary approach in physics is to be seen in the commonly accepted notion that at bottom, the world is constituted of a set of separately existent fundamental entities of a fixed nature (e.g. elementary particles) which serve as 'basic building blocks' for the whole of reality. . . . To help indicate how our world view has to change, we first note that the atoms originally thought to be the basic 'building blocks' have since been split into electrons, protons and neutrons. But these so-called elementary particles have in turn been shown to have an as yet very poorly known deeper structure, and to be capable of transformation, allowing for the creation and annihilation of an apparently unlimited number of further new unstable particles of a similar nature. It is clear that these particles are very unlikely to be ultimate and basic building blocks either. Thus far, the search for such basic constituents has failed; and indeed, the goal seems to retreat to the horizon each time that we think we are approaching it.

Later on he says (pp. 14-15):

> Even if we consider what is now known about the present so-called elementary particles, we can see that there are a number of important new developments, indicating that we have already to explore what is outside the 'plane' of the particle concept as a whole. Thus, the theory of relativity shows that these 'particles' cannot consistently be taken as the starting point of our reasoning. Rather, they have to be understood as abstractions from a stream of events or a flow of process, in which *every* object is regarded as in essence a relatively invariant form of such abstraction.

Thus Professor Bohm concludes (pp. 16-17):

> In this [new] view, there is no ultimate set of separately existent entities, out of which all is supposed to be constituted. Rather, unbroken and undivided movement is taken as a primary notion. Or, equivalently we can say: *What is* is a whole movement, in which each aspect flows into and merges with all other aspects. Atoms, electrons, protons, tables, chairs, human beings, planets, galaxies, etc. are then to be regarded as abstractions from the whole movement and are to be described in terms of order, structure, and form in the movement. The notion of a separate substance or entity is dropped, or at most, retained as part of the earlier world view, which is now seen to fit the totality of our experience only in certain limited ways.

Since Bohm's survey of 1971 further atomic fragments have been postulated, especially two hypothetical subunits called quarks, one of them being the antimatter counterpart of the other, an anti-quark. In recent weeks the newspapers have carried stories about newly observed particles with a lifetime of a fraction of a second, and their unifying characteristic seems to be what is called "charm."[8] So far charm does not seem to be divisible into countable units. The most advanced physics still seems to harmonize with the insight of Heraclitus.

How can we account for our feeling that we do perceive discrete units that appear to be semantic units? This feeling springs, I submit, from the workings of language. This view is often called the Whorfian hypothesis. I might hesitate to call this to your attention on the ground that it is "old hat," and yet in a recent issue of the magazine *Encounter* I find the statement by Hugh Lloyd-Jones, the Regius Professor of Greek at Oxford University, as follows: "Few linguists at present sympathise with Whorf; most accept the

universalising theory associated with the generative grammar of Noam Chomsky."[9] Perhaps, after all, Whorf needs reviving. The segmenting of the world, that is, the creation of objects, is accomplished by the imposition on it of language forms. This can be taken in a strong or weak form and readers of Whorf differ in their interpretation of his position. George Steiner says, for instance, in his *After Babel* (1975, p. 89): "Whorf is not altogether clear as to whether language determines that conceptualization or only conditions it." If we take only the weak form we may as well admit, as Max Black has said, that it "is too obvious to require mention."[10] But the strong form furnishes an underpinning for the search for semantic units. In view of the remarkable diversity of languages, it is a strong challenge to linguists to organize and methodize the varying linguistic systems. I was surprised to find that George Steiner says that scientific linguists resent the "mobile, perhaps anarchic prodigality of natural forms." I would say that linguists glory in the richness of their material. Certainly lexicographers do, and I would imagine that other workers do too.

Cross-cultural studies thus loom as especially important, and we can get further bearings on the degree to which we are prisoners of a particular culture. Our freedom is very much a matter of degree. Uriel Weinreich claimed much when he said (1963, p. 119): "We must carefully avoid the unjust claim that man cannot in his thinking transcend the 'logical mold' given by his languages; there is ample evidence to the contrary." This may be so; but still considerable *hubris* is involved in saying so. Just when a linguist says to himself, "Ah, how emancipated I am!" he may be missing some of the nuances and subtleties that are in the plenum of experience.

In the analysis of cognition that I am here proposing, the linguistic mechanisms have supreme importance. How do the formal units of a language operate? Technically speaking, the PHONEMES must be identified and then the next step is to isolate the MORPHEMES; but western cultures have given primacy to the units known as WORDS. Eugene Nida says in this connection in his recent book *Exploring Semantic Structures* (1975, p. 20): "A language needs a basic inventory of symbols which are relatively fixed in meaning and largely arbitrary. If they were too closely tied in form to their referents, they would be too rigid for the dynamic growth and movement essential to a living semiotic system."

There are many problems in identifying "the word" as a unit, and many linguists have pointed out that it does not have cross-cultural validity. Nevertheless, there is no doubt that the word carries great, even exaggerated, importance in our culture. The larger forms, though they are not basic semantic units in the sense used in this paper, must be investigated—the phrase, the clause, the sentence, the utterance.

While these units, especially the morphemes and words, operate as building blocks in their syntactic relations, they are quite different in their semantic relations. In those connections it is recognized that the units can be broken down by componential analysis. In this outlook, the semantic range of a form is made up of covert features that bundle together. The early investigations were made in the 1950s mostly by anthropologists, beginning with the area of kinship terms, as it was a productive way of comparing cultural outlooks. As John Lyons has said (1968, p. 472): ". . . the semantic components might be combined in various ways in different languages (and thus yield 'senses' or 'concepts' unique to particular languages)." Componential analysis reduces the notion of "thingness" in word-meaning, showing that "a meaning" is a summing up of features.

A recent development that is clearly allied to componential analysis appears in a brilliant study by William Labov, published in 1973 as "The Boundaries of Words and their Meanings." He tackled the problem of what features are criterial in establishing the definition of an object. He begins his analysis by saying (p. 342): "If linguistics can be said to be any one thing it is the study of categories: that is, the study of how language translates meaning into sound through the categorization of reality into discrete units and sets of units." He is not content to assume the nature of the classifications, but he attempts to

find out by empirical tests what are the regularities that determine the selection of features leading to denotation. As he describes it, he says (p. 347): "We will be dealing with the conditions which govern denotation, that is, the act of naming or reference which associates a linguistic sign with an element of the extra-linguistic world." He devised a number of experiments around a series of cup-like objects, using many sets of speakers, in order to find out what features caused them to classify an object as a "cup," or a "bowl," or a "mug," etc. He found that certain features formed an invariant core, while other features had a variable range. On this basis the lexicographer could frame a definition recognizing the criterial features and the degree of relevance of the other variable features.

Labov's study itself is remarkably cogent, making explicit what lexicographers have long been trying to do. However, at the end, in a codicil, he adds a section that I disagree with profoundly. He expresses a point of view, which he calls his "conviction," that (p. 368) "there is no significant difference between the distinctive/redundant opposition and the Aristotelian notion of essence and accident. A search for distinctive features is fundamentally a search for the Aristotelian essence, through which the thing itself is to be known. . . . Essence is opposed to accident, and reflects the way things really are, intrinsically, and cannot help being." This is a mixing of philosophical systems that I regard as utterly insupportable. It is incompatible to take an absolutist and a relativist position at the same time. In scientific pursuits, ecumenicism can be carried only so far. It may appear on the surface to be a humane stance, but it is stultifying to sound scientific procedure.

We have been plagued in linguistics with misleading terminologies, such as concepts rather than formulations, universals rather than generalizations, categories rather than classifications, deep structure rather than levels of analysis, reality rather than observed features of the environment, rule-governed behavior rather than habit patterns, intuition rather than generalized experience, the infinite rather than a very large number, and so on. Another important discrimination was firmly established in Hockett's monograph, *The State of the Art,* namely that the well-defined domains must be kept strictly separate from the ill-defined domains (1968, pp. 44-55). I am in agreement with Robert A. Hall's clarion call to us when he said (1969, p. 227): "Aprioristic rationalism and all its consequences, such as the 'innateness-hypothesis' and the establishment of 'rule-governed behavior' as a goal—must be returned as quickly as possible to the limbo of out-worn dogmas, and linguistics must return to its basis in observation of humans' activities in relation to their culture, if it is to continue developing as a science."

I believe that a responsible linguist cannot hold himself apart from making a commitment. I cannot agree with John Lyons when he states (1968, p. 443): "Acceptance of the structural approach in semantics has the advantage that it enables the linguist to avoid commitment on the controversial question of the philosophical and psychological status of 'concepts' or 'ideas.' " Is it really an advantage to avoid such a commitment? Our traditional language leads automatically to the old mentalistic view, and there will be no change in outlook without a definite decision to change terminology. If a linguist is to be a responsible scientist, he is under obligation to achieve as much rigor as he can. Rigor is possible only by leaving the traditional terminology behind us. I do not anticipate agreement among scholars on these matters, for the differences run deep in cultural history; but we can exhibit the candor of agreed-on disagreement and comport ourselves with gentlemanly civility.

Let us move forward now to examine some of the attempts to find basic semantic units. Earlier in this paper I have maintained, with Whorf, that the segmentation of the flow of the event level of existence takes place by means of linguistic mechanisms. The chief of these is the word, although that term needs refining and supplementation as linguistics moves forward. When words are taken out of context for study, they are no longer the same as they were in the human situation. They become set up as specimens

and thus are said to be "hypostatized." A dictionary entry, or an example in a semantic treatise, is a hypostatized form. Kenneth Pike made much of this term (1967, pp. 156-68 and elsewhere). A Finnish scholar, Arne Runeberg, has gone so far as to say that in the language of scientific logic, words have to be "killed" (1953, pp. 27-32). This rightly designates the move from an ill-defined system to a well-defined system, as logic purports to be; but still the figure of speech of "killing" words seems rather gruesome.

To counteract the shortcoming of hypostasis, an English school of philosophers has developed the notion of the "speech act" as a unit. As J. L. Austin, in *How to Do Things With Words* in 1955 declared (p. 138): ". . . what we have to study is *not* the sentence but the issuing of an utterance in a speech situation." This is not exactly new, as we all remember that Bloomfield led into his treatment of the use of language with the illustration of Jack and Jill, not getting a pail of water, but with Jack vaulting over a fence to get Jill an apple (1933, pp. 22-27). Austin's approach has been taken up by John R. Searle in his book *Speech Acts* of 1969. Searle presents it as follows (p. 16): "The unit of linguistic communication is not, as has generally been supposed, the symbol, word or sentence, or even the token of the symbol, word or sentence, but rather the production or issuance of the symbol or word or sentence in the performance of the speech act. . . . Speech acts (of certain kinds to be explained later) are the basic or minimal units of linguistic communication."

The speech act has a shortcoming as a semantic unit, in that it is not a discrete entity with clear boundaries. It is impossible to say where one act leaves off and another begins. But Searle links the speech act closely to concrete linguistic material, when he says (1969, p. 19): "for every possible speech act there is a possible sentence or set of sentences the literal utterance of which in a particular context would constitute a performance of that speech act." He emphasizes that these would be, in Saussurian terms, part of "parole" rather than "langue."

Even more fundamental than the speech act is a unit that has been called the "semantic reaction." This was set forth in 1933 by Alfred Korzybski as a basic term in his general semantics. He chose *semantic* to refer to the reaction of the organism-as-a-whole, without any split between emotions, feelings, intellect; in fact, a close synonym was "evaluative." The semantic reaction was fundamental, he felt, because it was neurologically based, and he declared (1933, p. 20): ". . . the neurological attitude toward 'meaning' is the only structurally correct and most useful one." To emphasize this, he frequently used the hyphenated word *neuro-linguistic.*

Both the speech act and the semantic reaction do not occur in a vacuum, and it seems to me that they could well be enlarged to the "speech situation," which would include enough context to be worthy of extended explication. A favorite example of mine, that I have often used in my classes at Columbia University, was found in a newspaper dispatch from Chicago to the New York *Times,* when Chicago's subway was being installed. It has the headline, "Climbs a Down Escalator: Stalls in Chicago Subway," and this is the account:

> The plight of elderly persons who are puzzled by the escalators in stations of Chicago's new subway was demonstrated last night by a woman about 65.
>
> She tried repeatedly to walk up a descending escalator at the Madison and Monroe station. Finally she managed to get up about six steps and, holding to the guard rails, kept pace with the escalator in treadmill fashion.
>
> A guard shouted to her to turn around. As the woman reached the bottom again, she collapsed. Revived after first-aid treatment, she said: "The old-fashioned stairways will suit me here hereafter."[11]

Here is a case, it seems to me, where a misevaluation was the result of the words whirling about in her head. She did not examine the event in front of her but followed the words given to her by her culture. On seeing an escalator for the first time, she classified it as

"stairs." She saw only what her language allowed her to see. She had known stairs all her life and stairs are meant to be walked up. If she could have looked at the situation without the mediation of language, she could have seen that a channel of material coming downwards in her direction was not the proper means for going upwards. However, her language classifying supervened over observation of the facts.

Another revealing speech situation is found in a "Dear Abby" column, widely circulated in the newspapers over the country. A contributor wrote in as follows:

> Dear Abby: . . . My husband and I have owned and operated a small florist shop for the last 15 years, and I have encountered some unusual situations. But the one I had yesterday beats them all.
>
> Two middle-aged women came into the shop together and asked what I had to offer in the way of a "Bon Voyage" wreath, so I showed them what I had in my catalog. I asked if a man or a woman was going abroad, and one of the women replied, angrily: "It is for my sister's FUNERAL!"
>
> I must have shown my amazement because she then added: "If you were a true Christian you would consider death as the beginning of a long, happy trip."
>
> Abby, I do consider myself a Christian, and I refused to sell that woman a "Bon Voyage" wreath for her sister's funeral.
>
> When my husband came in, I told him what had happened and he laughed and said: "You should have sold it to her."
>
> I was very much upset by my husband's response as I have always considered death to be a very solemn occasion. Maybe you or some of your readers could tell me what they would have done.
>
> [SIGNED]–SHOCKED

This situation has a very high linguistic content, with much cultural conditioning behind the terms *bon voyage, funeral, true Christian, happy trip, death,* etc.; and full significance can be understood only in the large social context.

A linguistic situation, even though it is basic for semantic analysis, is not a minimal semantic unit, but a complex structure of interrelated units. The search for a basic unit might well zero in on a simple fundamental unit of a syntactic nature. This has been done by Kenneth Pike in his TAGMEME. Another attempt was made by Bert Decker of the State University of New York at Buffalo. He presents us with a "VF," which stands for *verifiable function,* explained as follows (1969, p. 2):

> The *"verifiable function"* is an extremely handy part and the smallest meaningful part of verbal structure that I have been able to find. Realize that a word by itself cannot be a meaningful, scientific dimension or unit of language. A word by itself has no meaning. The *"verifiable function"* is a meaningful unit of verbal structure.
>
> The *"verifiable function"* consists of a verb and a noun. However, it is a unique verb and a unique noun and their uniqueness hinges upon their being together to form the meaningful structural unit. We can define the *verifiable function* as a demonstrable verb and a measurable or countable noun whose *combined* meaning can be demonstrated on the non-verbal level without using words except the verb and noun. Examples of verifiable functions are *"lift pail," "lower pipe," "reverse pencil," "underline noun"* and *"add weight."*
>
> To me, a "verifiable function" is the lowest common denominator of meaningful verbal structure. Most important, it is a *verifiable* unit of meaningful language. We can either demonstrate the meaning of a verb-noun function or we cannot. If we can demonstrate its meaning, if others can "see" its meaning, it is a verifiable function. It has the same scientific validity as the arbitrary inch whose meaning must also be demonstrated on the non-verbal level. The inch is an invented, arbitrary dimension of human measuring behavior. The *verifiable function* is an invented, arbitrary dimension of human verbal behavior.
>
> Because it is the first invented scientific verbalized dimension which has been intuitively used for centuries, the *"verifiable function"* allows us to define precisely many words important to creative behavior, science and education which have been plagued with vagueness and lack of precision for centuries. This advantage springs from the fact

> that the invention of the *"verifiable function"* allows us for the first time in history to count *meaningful* verbal units. Counting words was little help since a word by itself has no meaning. Being able to count *"verifiable functions"* can lead to surprising advantages. Most important, being able to use a meaningful verbal unit to define concepts is the only hope for clarifying many of them.

Despite Decker's sanguine tone, I have grave reservations about the soundness of his analysis. Whether or not he has something valuable depends completely on his making his "VF" stick. When he admits that it is "an invented, arbitrary dimension," I do not see how he can expect different investigators to make the same arbitrary segmentations. Such agreement is at the heart of "verification" and the method of science itself. In the contrast between the "hocus pocus" approach vs. the "God's truth" approach, it seems to me that he has some hocus pocus masquerading as if it were God's truth. He attempts to establish language in Hockett's "well-defined system," not recognizing that language involves the approximations of diverse human beings experimenting with sentences. His VF can never be fully V. I called some of these points to his attention and in a personal letter he replied:

> To me, as I define a "verifiable function" it is extremely handy because there is a precise line of demarcation between "verified" and "unverified." It is possible for you and me to agree what we mean (by demonstration) by *"lift pencil," "underline word," "lower hand"* etc. However, we have yet to learn how to demonstrate on the non-verbal level without using words other than verb and noun, such functions as *"glorify God," "detect thoughts," "display reason."*

In my own opinion, Decker's VF is a valiant attempt to find a unit, but it involves insurmountable difficulties.

Of great and fundamental importance is Pike's use of the TAGMEME. He has elaborated his "tagmemics" into a broad system that covers all of human behavior. His discrimination between *-etic* units and *-emic* units has had profound implications both within and outside linguistics. For the widest context he has developed the notion of the BEHAVIOREME (1967, pp. 120-49), and in some senses that is a basic semantic unit. Altogether Pike's three-part volume, *Language in Relation to a Unified Theory of the Structure of Human Behavior,* is a monumental achievement that deserves continued careful attention.

Hockett has developed an expanded framework for the description of communicative systems, in which the "plereme" is the basic formal unit. In his *Course in Modern Linguistics* he made use of the term *plereme* to describe a unit that is comparable to a morpheme in a limited language, and its meaning would be a semantic unit (1958, p. 575). He developed this system further in 1961 in his study "Linguistic Elements and their Relations." In describing the property of SEMANTICITY, he said (p. 45):

> In a semantic communicative system, messages bridge antecedents and consequences by virtue of associative ties between the messages in the repertory, or some elements in the messages, and things or situations or kinds of things or situations in the environments of the users of the system. The messages or message-elements that have such ties are PLEREMES; the environmental things or situations or kinds of things or situations are the DENOTATIONS of the pleremes.

He goes further to point out that some linguists, notably Zellig Harris, have attempted to isolate or discover morphemes by dealing with phonemes alone. If this were true, we would have no difficulty in deciphering Etruscan. An additional criterion must be added to phonological information, and as Hockett says (1961, p. 46), this "is always at bottom semantic, no matter how disguised."

I feel that in the long run it will be most fruitful to return to the analysis of Bloomfield and put the sememe in a central position for semantic analysis. In doing so, two papers are especially helpful: one is Eugene Nida's "A System for the Description of

Semantic Elements" in 1951 and the other, Sydney Lamb's "The Sememic Approach to Structural Semantics" in 1964, declaring the necessity of a "sememic stratum" that mediates between semantic data and a lexemic stratum.

As a basis for his systematic coverage, Nida stated (1951, p. 105):

> The meaning of a form is describable in terms of the situations in which it is used, i.e. in terms of its ethnolinguistic environment. We may prefer to talk about cultural contexts or to describe stimulus-response behavior, but fundamentally we are describing the meaning of a form in terms of its ethnolinguistic distribution. The ethnolinguistic environment comprises two types of contexts: (1) linguistic and (2) nonlinguistic (i.e. the biosocial environment, or "practical world").

Later he provided names for these, when he said (p. 106): ". . . we may identify a minimal feature of meaning based on the linguistic context as a *linguiseme.* This contrasts with an *ethnoseme,* which, as the name implies, identifies a minimal feature of meaning based on the ethnological context." He went on to give a full panoply of terminology, including *alloseme, episeme, alloepiseme, episememe, macroseme, allomacroseme,* and *macrosememe,* together with combinations of them, such as *linguimacroseme.* I cannot see that these are needed in our usual discourse; but they provide an elegant classification of the data.

In a later paper in 1964, "Linguistic and Semantic Structure," Nida advocated cutting semantic units loose from any referents, and considering only their relation within the linguistic system, that is, their "distributional meaning." He described this as follows (p. 15):

> In order to discover the semantic structure of a language we must be concerned with the semantic units and their distribution within the linguistic discourse, not with the referents and their classification in the non-linguistic world. The distribution which concerns us is not describable in terms of the practical-world context (i.e., the when, where, and how of the use of the referents) but in terms of the linguistic context (i.e., the when, where and how of the use of the semantic units).

It is of interest that when Nida re-published this essay in his book of 1975 he backtracked somewhat by adding this footnote (p. 194): "While distributional approach to the problems of differences of meaning of a single word is valid, it no longer seems necessary. In many instances one can approach the same problems more directly and efficiently by making use of the reference of terms to describe the componential features of the meaning." Thus he comes to incorporate the newer technique of componential analysis, about which he has written a detailed monograph (Nida 1975).

I regard it as very heartening that semantic analysis has taken tremendous strides in recent decades. It should be of some concern to us that the findings of a sound linguistics should have an influence in "popular semantics." I trust you will grant that popular semantics is worthy of our attention. It is to be hoped that people in general may come to recognize the shallowness of Max Lerner's dictum that "a word is the skin of a thought."

A revised linguistic outlook will tend to undermine the absolutism and certitude of word meaning. It must be pointed out that no two contexts are ever identical, and a different set of experiences are brought into play whenever a word is introduced into discourse. Lexicographical evidence supports the doctrine, stated in extreme form, that *no word ever has the same meaning twice.* Bloomfield's classical statement on this point was as follows (1933, p. 407): "Every utterance of a speech-form involves a minute semantic innovation." A similar conclusion can be quoted from Hockett, Pike, Nida, and others.[13] It is astonishing that a first-rate logician like Ernest Nagel is unwilling to admit this. As he has declared: "If this were really the case, no valid inference could ever be drawn and no inconsistency ever exhibited."[14] He is dead wrong about what is "really the case," although there is also enough stability in word meaning to allow the communi-

cation process to go forward. A valid inference can be drawn only in a well-defined system, when words have been killed and sentences hypostatized as propositions.

Another revision in popular semantics would have to do with the matter of precision in word usage. A careful precision can arise only when there is arbitrary agreement among speakers. Meteorologists will discriminate between *weather* and *climate;* as one of them has said: "weather may change from day to day but climate goes on all the time; it is the summing up of all the day-to-day changes of weather."[15] Similar discriminations may be made even in low-class speech. A social worker found in a poor district of Liverpool that a distinction is made between stealing from public property and from individuals. Two terms are used: "It is 'thieving' to steal from the 'stores or the like' but it is 'robbing' to steal 'from you or me.' "[16] In a commercial context, the discriminations may be self-serving. A mother bought a nonbreakable record but found that her four-year old after one playing broke it in half. She wrote to Columbia Records and received the following reply:

> Please be advised that our records are not *un*breakable, which means they will not break under any condition, but *non*breakable, which means they will not break under normal conditions and usage. We do not feel that we should be held responsible for your record's breaking. However, as a courtesy, we will replace it for you this time.[17]

There are uncertainties even in the "King's English," if we may believe a story told of King George the Fifth. When Margot Oxford was lunching with him and Queen Mary, he started to tell one of his naval stories and mentioned a cask of rum. Mary interrupted: " 'Keg,' George." And he replied: "No, Mary, 'cask.' " " 'Keg!' " said Mary firmly. " 'Cask,' " said George more irritably. They then appealed to Margot, who replied diplomatically: "Both seem to me correct, Your Majesties."[18]

What is needed most of all in "popular semantics" is an awareness of how deep language goes in determining our perceptions. If we accept the Heraclitean assumption, it follows that there are no objects in nature, but that they are created by the interaction of the flow of events with the human nervous system, operating by means of linguistic mechansism. The symbol creates the object. I would criticize a statement by a philosopher at Yale, Professor Frederic B. Fitch, when he said: "no amount of linguistic analysis can in itself provide a theory that will give an account of the structure and nature of nonlinguistic entities (e.g., living organisms, crystals, airplanes, etc.)."[19] What he does not realize is that by the time he perceives an entity, the linguistic mechanism has already been at work. The expression *non-linguistic entity* is a contradiction in terms. The world of process, the event level, is not differentiated into "entities" until human experience, partly individual and partly social, by means of abstracting, establishes these entities.

To be sure, the student of language as such cannot make investigations into the empirical fields mentioned by Professor Fitch, for each field falls within the purview of some specialized scientist. Each investigator must be his own semanticist; and his success and originality will result from his degree of semantic awareness and sophistication. The linguist can tell him how language works.

The sound linguist, in his awareness of ever-changing process, will be opposed to "thingness." There is good reason why, as Kenneth Pike has noted (1967, pp. 271-72), "the idea of unit itself has come under subtle attack." The outlook in glossematics has been strongly stated by Uldall (1957, p. 8): "To the scientific view the world does not consist of things, or even of 'matter,' but only of functions between things, the things themselves being regarded merely as points where the functions meet." The same view is held in the more general system of Alfred Korzybski, when he said (1933, p. 20): "In fact, even objects, as such, *could* be considered as relations between sub-microscopic events and the human nervous system." In stratificational grammar, the notion of "cognitive networks" presents a refinement of the search for units. Sydney Lamb has gone so far as to hold (1970, p. 205) that linguistic structure "does not have items at all, nor

does it have rules." A cautionary note was sounded by Archibald Hill (1962), who holds that within linguistics discrete units are a necessity. As he says (p. 345): "Any utterance is separable into discrete units, since only chunks and pieces can be arranged and manipulated. Continua can be blended and stirred, perhaps, but not manipulated."

The most promising direction for semantic analysis is to look to relationships and to study the configurations of forces that come together at particular points. In this way our outlook will be in harmony with the recognition of the unsegmented flow that holds sway in the universe.

FOOTNOTES

[1]New York *Post,* May 15, 1972, p. 33, col. 1.

[2]Chafe unfortunately retreated from his position set forth several years before in "Phonetics, Semantics, and Language," in *Language,* 38.335-44 (1962), in which he stated (p. 336): "An important characteristic of the universe is the fact that it is continuous, containing within itself no discrete properties."

[3]See Hockett 1948, p. 572. This follows the reasoning of Bloomfield 1933, pp. 142-44, and 1936, an article as trenchant and needed now as when it was written.

[4]Weiss's outlook is summed up in his book, *A Theoretical Basis of Human Behavior* (2nd ed.; Columbus, Ohio: R. G. Adams & Co., 1929), xvii, 479 pp. He and Bloomfield worked together closely at Ohio State University.

[5]*Language,* 20.246 (1944).

[6]Bernard Bloch and George L. Trager, *Outline of Linguistic Analysis* (Baltimore, Md.: Waverly Press, 1942), p. 5.

[7]Some theorists have in this connection made good use of the term *fiction.* See especially W. Freeman Twaddell, *On Defining the Phoneme, Lang. Monograph* No. 16 (1935). I have myself thought that Jeremy Bentham had this insight in his "Theory of Fictions" (see Dineen 1973); but Ogden and Richards forced Bentham into their own mentalistic mold.

[8]New York *Times,* July 14, 1975, pp. 1 and 15. See also Sheldon Lee Glashow, "Quarks With Color and Flavor," in *Scientific American,* 233, No. 4 (October, 1974), 38-50, much marred by whimsical word play. I am sympathetic to word play, but not in such a profound area.

[9]*Encounter,* 44, No. 6 (June, 1975), 65.

[10]Max Black, "Linguistic Relativity; the Views of Benjamin Lee Whorf," in *Models and Metaphors* (Ithaca: Cornell University Press, 1962), p. 253.

[11]New York *Times,* October 30, 1943, p. 30, col. 5. See also my discussion of the incident in Read 1962, pp. 329-30.

[12]New York *Post,* January 14, 1972, p. 42, col. 6.

[13]I have gathered such statements together in my study, Read 1973, p. 167.

[14]*Journal of Philosophy,* 38.721 (1941).

[15]*Countryman,* Spring, 1955, p. 153.

[16]Madeline Kerr, *The People of Ship Street* (London, 1958), p. 126.

[17]*New Yorker,* November 17, 1956, p. 45, col. 3.

[18]*New Statesman and Nation,* March 28, 1953, p. 361, col. 3.

[19]*Journal of Philosophy,* 35.360-61 (1938).

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'THE TEACHER TAUGHT THE STUDENT ENGLISH': AN ESSAY IN APPLIED LINGUISTICS

M.A.K. Halliday
University of Illinois at Chicago Circle and University of Sydney, N.S.W.

When we are faced with the problem of analysing a piece of text, whether in English or in some other language, we tend to assume that there is one, and just one, correct analysis, and that our job is to find out what it is. Given this or that particular clause, we ask what ***is*** its structure; and we are not satisfied till we have found 'the' answer.

Yet there is always more than one way of looking at things; and a sentence, in this respect, is just another thing. It has many sides to it. What is more significant, however, is that different ways of looking at a sentence may reflect different facets of the meaning of that sentence: a text is an act of meaning, and we usually mean more than one thing at a time. Different meanings may, in turn, stand for different modes of reflecting on and acting on reality.

Let us consider the following English sentence:

the teacher taught the student English

It is a simple sentence, consisting of one clause, and we might begin by analysing it as follows:

the teacher	*taught*	*the student*	*English*
ACTOR	PROCESS	BENEFICIARY	GOAL

When we analyse the clause in this way, we are relating it to all other clauses to which we would assign the same structure, for example *Peter gave Paul a penny*. In other words we are interpreting 'teaching' as being like 'giving', as a process having three participants, an ACTOR (the teacher), a GOAL (English) and a BENEFICIARY (the student). We could point to the fact that, if we re-ordered the parts so as to have *the student* at the end, we should have to add the preposition *to* in the usual way: *the teacher taught English to the student.* An acceptable paraphrase would be:

'the teacher imparted English to the student'

The structure, however, is simply the means of expression of the options (in this case options in transitivity) that the speaker has chosen to take up. It is the realization of a selection of systemic features. The particular systemic selection that is realized by the configuration PROCESS + ACTOR + GOAL + BENEFICIARY is:

material process : action / (non-middle : active) / benefactive

Here, then, is our description No.1. Line (a) is the systemic description of the clause, taken just up to that point in delicacy that is relevant to the discussion; this represents the meaning of the clause in grammatical terms. Line (b) is the structural description

which is derived from (a) and shows the configuration of elements through which the grammatical meaning is expressed. Line (c) gives an example of another clause having the same features and the same structure; and line (d) is a paraphrase designed to bring out what the clause is being said to mean if it is analysed in this way:

(a) **material process : action / (non-middle : active) / benefactive**

	the teacher	*taught*	*the student*	*English*
(b)	ACTOR	PROCESS	BENEFICIARY	GOAL
(c)	Peter	gave	Paul	a penny

(d) 'the teacher imparted English to the student'

What are the implications of this analysis, as an interpretation of the language teaching process? It suggests a view of language teaching as a transaction, as the handing over of a commodity to a recipient. The Process involves a giver, and a gift: the teacher, and the language. In terms of classical transitivity theory it is the presence of these two, the ACTOR and the GOAL ('logical subject' and 'logical direct object') which defines the true nature of the process; while the presence of a recipient ('logical indirect object') is incidental, being required simply to round off the transaction.

But this is not the only possible analysis of the clause. Here is description No.2:

(a) **material process : action / (non-middle : active) / bounded**

	the teacher	*taught*	*the student*	*English*
(b)	ACTOR	PROCESS	GOAL	RANGE
(c)	Peter	beat	Paul	at ping-pong

(d) 'the teacher instructed the student in English'

In this interpretation we have a rather different model of the language-teaching process. The teacher is now doing something to the student rather than to the language. The action is directed to the student—although it is still the teacher, as ACTOR, who is setting the pace. The student is now a central element in the process; but his role is still a passive one – he is not doing something, something is happening to him. The language being taught has now become what we are calling RANGE, a bounding or delimitation of the scope of the process: the 'logical cognate object', if we want to preserve the 'logical' terminology. The teacher taught the student; the occasion for the teaching was English, just as ping-pong was the occasion for Peter's defeat of Paul.

Let us see how these interpretations are reflected in language-teaching practice. The first suggested a transactional view, with a giver and a gift as the leading roles. Here the recipient's role is an ancillary one, except that the giver presumably wishes to please the recipient, so the goods must be of the highest quality and nicely packaged and wrapped.

So we give him the best English available. It is, of course, ***our*** idea of what is best, not his, for it is rare for a giver to operate with the recipient's scale of values rather than his own. We give him Shakespeare and T.S. Eliot, even if what he would value most highly is the motor mechanic's manual; and we wrap it up in the best critical scholarship

we have, sometimes barely disguising our suspicion that it is really too good for him. It would be worse, perhaps, to give him the motor mechanic's manual if what he wanted was T. S. Eliot; but this does not often happen. Either way, the gift is an act of charity; and the student is invited to take it, or leave it. It is the language that is under focus of attention—that is the goal of the process, in grammatical terms.

By contrast, the second interpretation suggests a view that is interactional rather than transactional: the leading roles are now those of teacher and student. There is a suggestion here of a teacher of strong personality, acting positively on the class, while the language functions as a channel for the interaction, the means whereby the relation between teacher and student is expressed, as it were. There are frequent drills and exercises, and plenty of tests; the teacher is working on the student—instructing him in fact—and something must be seen to be happening. The student provides the responses (or the class provides choral responses, if the environment is one which favors that kind).

These two somewhat different conceptions of the language-teaching process both find their symbolic representation in the grammar of this clause. One suggests the analogy of consigning a commodity; the other that of manipulating a person; and both correspond to the grammatical facts.

But a language is not a commodity; and this is no doubt why, in the first approach, what is offered is usually something else: typically, literature, especially great literature of a bygone age. This is known in the trade as 'the literature fallacy' in language teaching. And learning does not take place by manipulation; as a social process it is interactive rather than responsive. However, if there is more than one way of analysing a clause it is likely that no one analysis tells the whole truth; and it would be a mistake to suppose that these models of the language teaching process have no validity at all simply because they do not by themselves satisfy our notions of what learning is, or of what a language is.

It is time to consider description No. 3:

(a) **material process : action / (middle : causative) / bounded**

	the teacher	*taught*	*the student*	*English*
(b)	INITIATOR	PROCESS	ACTOR	RANGE
(c)	Peter	got...to practise	Paul	palmistry

(i.e. Peter got Paul to practise palmistry)

(d) 'the teacher caused the student to learn English'

This puts the matter in rather a different light. Now, the student is seen as the 'doing' one; he is learning English, and the teacher is helping him to achieve this desirable end. The term 'causative' simply means that the agent in the process, or INITIATOR as we have called it, is distinct from the ACTOR: the student is doing the learning, but the teacher is bringing it about. We will ignore here the finer distinction between making, causing and enabling—between 'made the student learn', 'caused the student to learn' and 'enabled the student to learn'—except to remark that, since it is impossible to ***make*** anyone learn anything if he does not want to, if we paraphrase as 'make' then strictly speaking we should say 'study' instead of 'learn'.

This view of language learning suggests programming and mechanization, the use of a full battery of audiovisual aids. The student is the actor in the process; he is doing the work. The teacher comes in as helper, enabler; and in this role he brings with him all the modern techniques and facilities that are available for the purpose.

This model is probably closer than either of the earlier ones to the outlook of many teachers of English today. They would think primarily in terms of the student; seeing their own role as that of guiding and helping him in his task. But up to this point, we have not considered what the nature of the learner's task actually is.

There has been one assumption common to all three of the interpretations given so far: we have taken it for granted that the clause being analysed belonged to the class of material process clauses, and more specifically to those of the action type. Now teaching may well be regarded as a form of action, if only because it leads to physical exhaustion. But there are other types of process in the semantics of English; and where 'teach' is interpreted as 'cause to learn', it may not follow that we would regard learning in the same light.

Certainly *he is studying English* could be regarded as a clause of action. But *learning English* does not mean the same thing as *studying English;* and *teach* is related to *learn* rather than to *study.* Suppose we interpret *teach* as 'cause to learn' but treat 'learn' as a mental rather than as a material process, in the sense of 'come to know'; this is represented in description No. 4:

(a) **mental process : cognition / (middle : causative) / bounded / perfective**

	the teacher	*taught*	*the student*	*English*
(b)	INITIATOR	PROCESS	COGNIZANT	RANGE
(c)	Peter	interested	Paul	in politics

(d) 'the teacher enabled the student to come to know English'

The COGNIZANT is the central role in a mental process clause, the (typically human) participant in whose consciousness the process takes place.

By interpreting the clause in this way, with 'teach' as 'bring about a process of cognition', we have related it to a different and rather more specifically defined set of other clauses. This analysis retains from No. 3 the notion that teaching someone means enabling him to learn. But it treats learning as something different from the acquiring of skill in action. Learning a language, in this interpretation, is not like learning to drive, or learning bricklaying or acrobatics; it is a mental exercise involving the acquisition of knowledge.

Here the focus is again on the learner. The student is the key participant; the teacher is assigned the role of helper and guide. What is the student envisaged to be doing? He is enlarging his mental powers by the acquisition of new knowledge. And since the teacher is helping him in the process, the teacher's part is to supply the required knowledge, in doses of suitable size and strength.

The joker here is the word *knowledge.* It has often been pointed out that there is a difference between knowing a language, in the sense of being able to speak and understand and read and write it, and knowing ***about*** a language—knowing its rules, and being able to read and write up a grammatical description of it or compile a dictionary. The view of the learning process suggested by description No. 4 fails to make that distinction. It corresponds, in other words, to what is now called 'the linguistics fallacy' in language teaching (it used to be known simply as 'teaching grammar'): the tendency to teach *about* the language—that is, to teach linguistics—on the assumption that this is a means of (or even the same thing as) teaching the language.

This is a very old approach. It was no doubt taken over from the teaching of classics—which, however, is a very different kind of process, and always was so even when Latin was a living lingua franca. Its use in modern language teaching was denounced a hundred years ago by one of the pioneers of language teaching theory, François Gouin.

But it continues to flourish, and gains new life each generation as new linguistic theories come along. There are shifts of emphasis, and changed ideas about language; but whether the emphasis is on the facts of the language or on the principles behind them, and whether the grammatical ideas are those of Nesfield, Fries or Chomsky, the basic approach is the same. Learning a language is treated as a process of acquiring knowledge, like studying history or mathematics.

But when we talk of knowing a language, we are usually not talking about a knowledge of facts or principles. Even if learning a language seems to belong to the class of mental rather than material processes, it may be rather different from mastering a subject, such as history or mathematics. In No. 3, 'learn' was interpreted as 'acquire a skill'; only there the skill was assumed to be a skill in action. There is another possible analysis, one which also embodies the notion of acquiring a skill but treats this as a skill of a special kind, neither material nor mental but communicative. Here is description No. 5:

(a) **verbal process : (middle : causative) / bounded / perfective / potential**

	the teacher	*taught*	*the student*	*English*
(b)	INITIATOR	PROCESS	SPEAKER	RANGE

(d) 'the teacher enabled the student to become a speaker of English'

In this analysis, knowing a language means having the ability to communicate in it. The role of SPEAKER is equivalent to 'communicator', one who possesses and uses a verbal or other communicative potential. This is not unrelated to a cognitive potential, but it has a strong component of 'doing' in it; its uniqueness is proclaimed in the grammar by the existence of a special clause type, that of verbal processes, having distinct syntactic properties of its own. Here 'teach' is still 'cause to learn'; but 'learn English' is now interpreted not as 'come to know English', with its implication of knowledge *about,* but as 'come to be able to speak and understand English'. This expresses the complex notion that teaching a language means helping the student to master the ability to communicate, to develop a potential for meaning in that language.

A feature of this analysis is that it applies rather specifically to just this kind of process, namely 'teach + language', which it interprets as, in effect, unique. This is why it is not easy to find a clause having the same features in which Peter is not already teaching Paul some language. We might find a parallel in the imparting of special linguistic powers, technical or inspired, as in *Peter primed Paul in the language of the law,* or *Peter persuaded Paul to woo the poetic muse;* but these would hardly make matters any clearer—we have so narrowed down the class of processes that any differences within it become contrived—so we left row (c) out.

This will be the last of our excursions into the grammar of the language-teaching process. We have not attempted to justify or explain the analyses in detail; nor have we given more than a fragment of the full description, but only as much as is needed to bring out the similarities and the differences. In moving from one description to the next we have readily changed more than one feature at a time, ignoring other possible combinations in order to select just those which seem to suggest real interpretations of the process of language teaching. But each of the five descriptions presents a reasonable and defensible account of the clause *the teacher taught the student English.* Certain aspects of its grammar may be brought out more systematically in one version than in another; but no version can be dismissed as wrong, and all have something to recommend them from a grammatical point of view.

This clause is, perhaps, something of a syntactic oracle. Up to a point, the same is true of all clauses, although this is perhaps one of the more versatile ones. It is probably

the simplest and most typical expression of the process of language teaching that one could find in English. Yet it represents this process in a way which makes it look something like conveying goods to someone, something like working on him, something like helping him acquire a skill, and something like supplying him with knowledge; while at the same time being unique, differing in some respect from processes of every other type.

If one was forced to choose just one of these five interpretations and no other, one might be tempted to choose the last. Here language is seen as a potential; to learn a language is to develop that potential, and to teach a language is to help the student to develop it for himself. But it is a potential the mastery of which is different from other learning tasks. It is a meaning potential; to have learnt a language is to have mastered the ability to mean. This suggests an approach in which the teacher attempts to structure the language learning, within the limits of the external conditions and resources available, so as to impose on the learner specific demands that both relate to his own particular language learning goals and also stretch his cognitive muscles to the full.

However, if the linguistic representation of the language teaching process is so many-sided, it is likely that the process which it describes is many-sided too. No doubt the most effective language teaching is of a kind that is not tied methodologically to any one interpretation of the teaching and learning process. No doubt, too, in a class of thirty students there are thirty different ways of learning a language; and while these will be shaped and perhaps largely determined by sociocultural factors, it would be surprising if any one conception of the language teaching process was equally valid for all learners at all times in all places. Each one of the grammatical descriptions of the expressing clause reveals some aspect of its semantic origins; so it is not surprising if the process it expresses turns out to be one that is capable of being interpreted (and carried out) in so many different ways.

In conclusion it might be said that this has been not so much an essay in applied linguistics as an essay in social semiotics. We have taken a sentence about language teaching, consisting of one clause specifically referring to the language teaching process; and we have interpreted it, first in linguistic (grammatical and semantic) terms, and then in terms of its semiotic context, that of language teaching as a socially recognized act. If there are five different linguistic interpretations, they might correspond to different interpretations of the act itself; so we can go on to consider the nature of the language teaching process as it actually takes place. Behind the ***linguistic*** semiotics (i.e. the semantics) of this sentence lies the semiotics of the language teaching process. If we look at the ways in which people teach a language they are, typically, a mixture of a number of different conceptions of the teaching and learning processes and of the role relationships involved. This provides the social context in which we interpret this sentence, as well as, of course, innumerable other sentences that are about the same process. We have tried to take this further step in interpretation, going above and beyond the semantic system to the social semiotic, the systems of meaning that constitute the culture and are realized, inter alia, through language. In this instance the relevant semiotic domain is that of language teaching theory and practice—which can be related, in turn, to deeper ideological patterns within the social system. Each of our grammatical analyses of the clause symbolizes some set of assumptions, together with an associated methodology, that is recognizable in a world where, among all the other happenings, teachers do teach students English.

FOCUS AGAIN

Helmut Esau
Texas A&M University

Many recent linguistic discussions make reference to the concepts focus, topic, and topicalization, and some even rely on arguments from these areas to support a given linguistic analysis or to justify a particular linguistic model.[1] It might seem at first as if these concepts are relatively well defined and present no real problems to linguists. But a closer inspection of the literature leads to quite different conclusions.

1. Chomsky's position reexamined: Chomsky (1972), for example, discusses focus at some length in support of what has come to be known as the extended standard theory (EST). Chomsky claims that there are basically two alternatives for treating focus: either focus is determined 'directly from the deep structure, in accordance with the standard theory, the focus being the predicate of the dominant proposition of the deep structure', or 'focus is determined by the surface structure, namely, as the phrase containing the intonation center' (p. 90). Later in the discussion Chomsky emphasizes that 'these notions [focus and presupposition] seem to involve surface structure in an essential way, and thus to provide strong counter-evidence to the standard theory, which stipulates that semantic interpretation must be entirely determined by deep structure' (p. 101).

Chomsky's discussion raises a number of questions. Beginning with the least important one, we could note that there are grave doubts about whether or not the concept 'normal intonation' or 'normal stress' has been meaningfully defined, as Schmerling (1974) has recently pointed out. Although Chomsky is fully aware of this,[2] as is McCawley (1973) in his response to Chomsky (1972), both argue, nevertheless, as if this problem does not exist. But more disturbing still is the kind of evidence Chomsky uses to empirically justify his position. A few examples will demonstrate my point. (The reader may also wish to refer to McCawley's (1973) rather detailed criticism of Chomsky's treatment of focus.)

Chomsky recognizes (as did many linguists before him) that there is a correlation between focus and intonation center. He presents several pieces of evidence for this correlation and argues convincingly that no similar correlation exists between focus and the specific deep structures he assumes. From this he concludes that at least some aspects of focus have to be relegated to the surface structure.

But how did Chomsky arrive at that conclusion? He begins his discussion of focus by delineating the two alternatives available

to the transformational linguist: either focus is determined 'directly from the deep structure' or it is 'determined by the surface structure' (90).[3] (I will argue below that there are other alternatives, even in the TG (transformational-generative) model of grammar). Clearly, Chomsky uses a specific linguistic model to determine the treatment of focus in the first place. Yet he does not hesitate to offer his analysis of focus also as support for the particular model he prefers.

It is easy to see why Chomsky sets up this dichotomy, for the model itself clearly suggests it. Let us remember that Chomsky (1957) and (1965) proposed a model of grammar in which syntax was autonomous, semantics was interpretive, and all meaning was derived from the DS, which was considered a well-defined level of syntactic structure. As more and more evidence accrued suggesting that some aspects of meaning could not be handled in this manner, there was only one way to partly save the sacred dictum transformations do not change meaning: one would have to show that certain (well-defined) aspects of meaning were determined by the surface (or derived) structure.

Thus, the linguistic model not only outlined the areas where the empirical evidence might be found, but it also helped eliminate other potential alternatives. One is reminded of what Feyerabend (1968) says about the 'relative autonomy of facts' and consequently about the use of facts as empirical evidence. According to Feyerabend

> facts and theories are much more intimately connected than is admitted by the autonomy principle. Not only is the description of every single fact dependent on some theory (which may, of course, be very different from the theory to be tested). There exist also facts which cannot be unearthed except with the help of alternatives to the theory to be tested, and which become unavailable as soon as such alternatives are excluded (p. 27).[4]

In other words, the suspicion arises that Chomsky--as well as others--is guilty of a certain degree of circularity.

Let us next examine a number of Chomsky's specific suggestions. The statement that surface structure, and the intonation center in particular, determines focus is meaningful only if viewed in the context of a model of grammar where semantics is interpretive. As a general statement of a native speaker's linguistic competence, that claim is meaningless and, in fact, counterintuitive, as I have been assured by a number of native speakers of English. If, instead, we take the intuition of the native speaker into account, we would probably have to say that focus determines the intonation center (among other things).

Chomsky can, of course, brush this latter criticism aside by simply pointing out that he wants to describe the competence of an idealized speaker-hearer. Since this is obviously an abstraction, the description need not have psychological reality for either speaker or hearer. Without entering into a detailed

discussion of this problem, I simply wish to reiterate what I have said before in Esau (1973) and (1974a), namely, that

> Chomsky's positing of a speaker-hearer competence independent from the actual performance has permitted him to set up a buffer, as it were, between his description, which must not meet the criterion of psychological reality, and the performance-based empirical language data ... For while such data may correspond to the psychological reality of the speaker, Chomsky can always respond that his aim is not to give a psychologically real description but rather one that captures the artificially set-up competence of the artificially invented ideal speaker-hearer (1974a:5, 6).

I argue that a grammar of a language which, according to Chomsky (1968), is 'a hypothesis concerning ... [the] internalized system ... of rules that relate sound and meaning' (26) must also meet the condition of psychological reality and thus become verifiable by performance-based empirical data. I believe, furthermore, that we must distinguish between grammars of speech production and grammars of speech perception. (See also Chafe's (1970) discussion of this point.) One cannot simply proclaim the Humboldtian view: 'speaking and understanding are ... [two different] manifestations of the same underlying capacity, the same generative principle, mastery of which provides the speaker-hearer with the ability to use and understand all of the infinite range of linguistic items' (Chomsky 1964:57). But Chomsky has assumed the a priori correctness of this claim and relegated it to the metatheory, despite evidence to the contrary.[5]

These and similar criticisms cannot simply be brushed away by pointing out that the linguist's goal is to describe the <u>competence of an ideal speaker-hearer</u> and that critics fail to <u>perceive the difference between competence and performance</u>. For the <u>ideal speaker-hearer</u> concept is responsible, I believe, for much <u>of the circularity</u> in linguistic descriptions: it has neutralized our most important empirical evidence, the performance-based language data, and it has made it appear plausible to speak of a competence that is neither psychologically real to the speaker nor to the hearer.

Chomsky cites <u>empirical</u> evidence, to be sure. But the facts he uses are anything <u>but autonomous</u> (cf. Feyerabend): they stand or fall with the still (<u>or again?</u>) very hypothetical TG model. Chomsky's (1972) cleft sentence evidence is a case in point. First, we have to accept that cleft sentences are derived via transformation from the non-cleft sentences (or vice versa). Now, when more and more linguists are seeking alternatives to transformations, it is becoming less and less clear what sort of creatures transformations are. If I were to use my intuition as a speaker and as a linguist, I would have to reject the <u>derivational</u> concept of transformations. That is, I agree with Chomsky that speakers are aware of the relationship between an active and the

corresponding passive sentence, but I disagree that there has to exist a derivational relationship between the two or a common underlying form. This does not mean, however, that the speaker cannot derive, say, a passive sentence from its active counterpart.[6] Many operations stated as transformations may simply not be vertical relationships, as in Fig. (1), but rather horizontal relationships, as in Fig. (2):

(1)
$$\begin{array}{c} PM_1=DS \\ \downarrow T_1 \\ PM_2 \\ \downarrow T_2 \\ PM_3 \\ \vdots \\ \downarrow T_3 \ldots T_{n-1} \\ PM_n=SS \end{array}$$

(2)
$$\begin{array}{ccccc} PM_1 & & & & \\ \downarrow & & & & \\ PM_2 & \xleftrightarrow[T_i]{} & PM_{2'} & \xleftrightarrow[T_{i'}]{} & PM_{2''} \\ \downarrow & & & & \\ PM_3 & \xleftrightarrow[T_j]{} & PM_{3'} & \xleftrightarrow[T_{j'}]{} & PM_{3''} \\ \downarrow & & & & \\ PM_4 & & & & \end{array}$$

The value of this kind of distinction for a description of styles is obvious. But as long as empirical data do not have to meet the constraint of psychological reality, we have no way of proving with those data that Chomsky's position is untenable. I suggest that the limitation is not in the data but in the theoretical stance taken. I see no point in discussing either Chomsky's cleft sentence evidence or his hypothetical cleft sentence proposal in any detail, since McCawley (1973:37ff.) has already done so. McCawley argues that clefting is not a reliable test and that Chomsky's arguments have little substance, even within the framework of his own model. Moreover, McCawley points out that there are also problems with Chomsky's claim that the focus is always a surface constituent, as can be seen from sentences such as (3):

(3) Did he look the plans over?

where the italicized look ... over is clearly not a surface constituent. Yet, it was just this inability to relate focus to constituents in DS that led Chomsky to argue for the SS alternative.

But how can we avoid such questionable and in part circular discussions of linguistic concepts such as focus? I suggest that we first of all refrain as much as possible from defining and describing a linguistic concept in terms of one particular model

of language and simultaneously using the concept so defined to justify that model. Instead, we should first try to arrive at a working definition independent of any one particular linguistic paradigm. After we have established what might be called the lowest common denominator, but only then, should we attempt to incorporate this preliminary model-independent description into specific linguistic models.

2. Concepts defined: Next I attempt to provide a working definition for focus and the closely related concepts topic and topicalization. I shall reject the concept ideal speaker-hearer and be concerned with speech production only.[7] I will further insist that linguistic descriptions that intend to capture the speaker's competence must also possess psychological reality for the speaker.

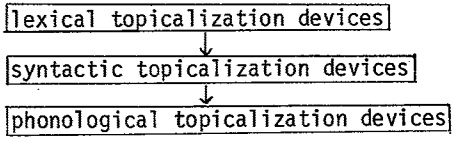
In contrast to Chomsky, I argue that focus is not determined by any one grammatical component: rather, its realization is accomplished through the interaction of all components of the grammar. Focus is not part of any single proposition; it rather makes possible the combining of different propositions into a coherent discourse. Let us say, then, that focus is a purposive concept and define it as that element of a proposition that the speaker establishes as the center of attention. Since the center of attention always depends on the context, we can add that focus relates the individual sentence to the surrounding discourse.

Since every proposition can be placed into many distinct discourses and since, moreover, each discourse may in turn require a unique propositional focus, the grammar must have devices capable of realizing all needed focus possibilities. I will use the term topicalization to refer to the realization of focus through the interaction of lexical, syntactic, and phonological (i.e. suprasegmental) devices.

An inspection of the different topicalization devices (TD's) suggests that focus selection is not a random process, as is so often implied. Rather

(i) TD's are hierarchically arranged, each step in the hierarchy coinciding with one of the three grammatical components (lexicon, syntax, phonology) as follows:

(4)



(ii) There exists an interrelationship and interdependency between the TD's of the three components.

Let us begin by examining the lexical TD's. In the lexicon, verbs are clearly marked for the element of the proposition that is normally topicalized. Thus, the English speaker knows that with

causative verbs, like kill, lift and throw, the agent NP normally becomes the topic. Linguists have, of course, accepted this fact for a long time,[8] yet its significance for focus selection has not been fully appreciated.

But, first, we need to define one more term: topic. Topic, in contrast to focus, is not a purposive concept. It is simply the lexically established provision for focus realization within any given proposition. The head of the proposition, that is, the predicate, carries with it a provision for focus selection. It is the predicate's recommendation, as it were. This focus provision, which is inherent in the propositional head, constitutes what I call the topic, or even better, the lexical topic.

Chafe (1970) takes a position very similar to the one I am advocating here. He shows that, given certain verb roots, the distribution of what he calls old and new information is partly predictable. He observes that 'a sentence ..., if it contains a noun at all, will always contain one and only one noun root which is not new--which conveys old information' (p. 217) and that 'there is a strong correlation between old information in semantic structures and subject in surface structure' (p. 213). There is, thus, a very basic correlation between sentence topic and old information and between predicate and new information.

My use of the term topic is closely related to the use of topic in referring to what is sometimes called the psychological subject. There is, however, one important difference. The traditional topic/comment division refers directly to the proposition to be communicated. The topic is what the speaker talks about and the comment is what he says about that topic. Without denying that this distinction is important, I will use the term lexical topic to describe that grammatical element which is most naturally elevated to the position of sentence topic for any given predicate.

3. Topicalization devices identified: The lexical topic is thus merely the lexicon's recommendation for focus selection with any given predicate. Whether or not the recommended element is actually selected as sentence topic is an entirely different matter, for every proposition can be used in many different discourses; and each discourse in turn may require a unique propositional focus. In other words, focus realization is not a random process, accomplished by the speaker's randomly choosing from among the various TD's. Rather, it is tied to the individual proposition with its recommended topic determined by the propositional head. I claim, then, that the lexical topic serves as the background on which focus selection is carried out. If we recognize that the lexical topic of a given predicate becomes the point of reference for all other TD's, we can easily explain how the speaker can select the appropriate TD's that will enable him to realize any focus imposed upon a proposition by the discourse, and do it unerringly.

The lexical topic of a given predicate remains constant from one use of that predicate to the next. It is, thus, fully

predictable, as any standard must be. Symmetrical predicates, have, of course, not one but two topic choices. But languages tend to create, even for symmetrical predicates, lexicalizations where the notion lexical topic is made explicit. Thus, besides similar and different as used in sentences (5) and (6), English has also the corresponding predicates similar to and different from as in (5') and (6'):

(5) Jane and Mary are similar.
(6) Jane and Mary are different.
(5') Jane is similar to Mary.
(6') Jane is different from Mary.

The lexicon does, however, give some options for focus selection by providing two or more lexical items with the same range of meaning but with a different lexical topic. An English speaker can, for example, choose between possess and belong to, depending on the focus required by the discourse. But sentence (8) does not illustrate a reversal of the lexical topic of (7) in the same way that sentence (10) represents a reversal of the topic of sentence (9):

(7) John possesses three brand-new cars.
(8) Three brand-new cars belong to John.
(9) Alexander Bell invented the telephone.
(10) The telephone was invented by Alexander Bell.

Although the effect of replacing possess with belong to is superficially the same as replacing the active form invented with its corresponding passive, there are, nevertheless, two important differences between the two processes. First, by replacing the lexical item possess with belong to the lexical topic of possess is not simply reversed: belong to establishes its own lexical topic, which serves as the point of reference for the syntactic and phonological TD's. A syntactic reversal of the topic, similar to that between (9) and (10), may be demonstrated by the pair (11) and (12):

(11) John owns three brand-new cars.
(12) Three brand-new cars are owned by John.

The syntactic process passive, rather than establishing a new lexical topic for the verb own, merely superimposes a new topic onto the topic inherent in the verb own. A second difference is evident in the greater productivity of the syntactic TD's. Given the appropriate structure ($X\ NP_1 + V + Y + NP_2 + Z$), passivization is possible regardless of the meaning of the sentence, assuming, of course, that the verb permits passivization. Lexical topic options, such as that between possess and belong to, are usually less general. Notice that it is not always possible to replace one with the other, as indicated by the ungrammaticality of (14):

(13) Richard possesses a high intelligence.
(14) *A high intelligence belongs to Richard.

I believe that the need for topic options is one important reason for the creation of lexical items. The verbs and adjectives that Lakoff (1970:126) wants to relate via a transformation Flip are similar to the lexical pair possess/belong to. Notice that certain idiosyncratic constraints would have to be built into such a Flip-transformation. While Lakoff's examples, (15)-(18), appear to work, counterexamples are not difficult to find; cf. sentences (19)-(22):

(15) What he did amused me.
(16) I was amused at what he did.
(17) What he had done pleased her.
(18) She liked what he had done.
(19) I liked Mary very much.
(20) *Mary pleased me very much (as a paraphrase of (19)).
(21) I enjoy the clown.
(22) *The clown is enjoyable to me.

I propose that amuse and amused at, like and please, and enjoy and enjoyable to are lexical entries in their own right. The existing parallels can just as easily be expressed as lexical relationships.[9]

However, the pairs which Lakoff wants to relate via T-Flip differ in yet another way. This becomes evident when we form the imperative, as in (23) and (24):

(23) Amuse him!
(24) *Be amused at him!

The inability to form the imperative form in (24) suggests that the change amuse ⟹ amused at involves also what I have called in Esau (1974c) an aspect change from process to state. Notice that many of the ungrammatical sentences containing the preposition at can be salvaged if by is substituted, as in the following examples:

(25) The irony amuses me.
(26) I am amused $\left\{\begin{matrix}\text{*at}\\ \text{by}\end{matrix}\right\}$ the irony.
(27) The comedian amuses me.
(28) I am amused $\left\{\begin{matrix}\text{*at}\\ \text{by}\end{matrix}\right\}$ the comedian.

Yet sentences (26) and (28) cannot simply be explained as an idosyncracy in the use of English prepositions. Rather, we have here two different processes, the at-sentence being the result of using the stative predicate amused at, the by-sentence being the passive sentence of the process entry amuse.

Notice, incidentally, that it is not unusual to find speakers

who are unable to distinguish correctly between such lexical pairs as own vs. belong and teach vs. learn, because they have not identified the lexical topic correctly. But when the topic reversal is accomplished syntacticly (e.g., via passivization), such a confusion of two topic possibilities rarely occurs.

The lexicon, then, provides the speaker with a focus recommendation for every predicate. In addition, a limited number of focus options are due to double lexicalization of the same conceptual structure, but with different lexical topics, leading to such lexical alternations as possess/belong and like/please. Further investigation may reveal that the apparent exceptions to general syntactic topicalization processes are due to interference from another area of the grammar. For example, the unacceptability of the passive of the sentence

(7) John possesses three brand-new cars.

is obviously due to the interference of the second sense of possess, as illustrated in sentence (29):

(29) Bill was possessed by a demon.

Syntactic TD's are thus relatively flexible and productive, if compared to the lexical topic options; they are, nevertheless, not nearly so flexible as the phonological TD's. The speaker can realize a host of different focus possibilities by simply shifting the intonation from one element of the surface sentence to another. Consider the following sentence taken from Chomsky (1972):

(30) He was asked to look out for an ex-convict with a red shirt.

Sentences (30 a-d) represent some of the different focus possibilities that can be achieved with different intonation contours:

(30) (a) He was asked to look out for an ex-convict with a red shirt.
(b) He was asked to look out for an ex-convict with a red shirt.
(c) He was asked to look out for an ex-convict with a red shirt.
(d) He was asked to look out for an ex-convict with a red shirt.

4. Hierarchy of TD's in the grammar: The evidence presented, thus, suggests the following sequence for focus realization:

i. The lexicon sets up the norm against which the selection of the remaining TD's is carried out. The notion lexical topic, which is here viewed as the focus recommendation within a given proposition, defines that norm.

ii. The syntactic TD's are much more flexible than are those provided by the lexicon. Whereas lexical topic options involve single verb entries, as in the case of possess/belong, or small and often highly idiosyncratic lexical classes, as the flip-predicates, syntactic processes such as passive, extraposition, clefting, etc., are much more productive. The difference is clearly seen in the way these different processes are usually described. Clearly, in order to describe the class of predicates that can undergo flip, it is necessary to list every form individually. But a syntactic process, such as passive, seems to look primarily at the syntactic structure of the input string. The most plausible treatment is, therefore, to let the passive apply freely whenever the appropriate structural description is met and to simply mark those forms which cannot undergo the expected passive transform. Depending on the desired conceptual focus, the speaker either selects the appropriate syntactic TD's (i.e., surface case assignment rules or the like) to realize the lexically recommended topic on the surface, or he reverses the lexical topic via such well-defined syntactic processes as passive, clefting, etc.

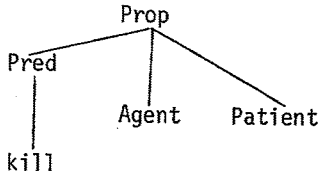
iii. The phonological TD's in turn can either follow the path selected by lexicon and syntax, or they can shift the focus to any number of other surface constituents. (See sentences (30 a-d).) Intonation is the most flexible TD of all, seemingly equipped to handle any focus selection that the discourse could require.

The sequence of topicalization devices can be demonstrated with sentence (31):

(31) The ex-convict killed the guard.

The verb kill belongs to the class of two-term action predicates that may become the head of a proposition such as (32):

(32)



The lexical topic of kill is the agent NP. If the lexical focus recommendation is realized at the surface, we derive sentence (31). However, the syntactic topicalization devices may reverse the focus in certain well-defined ways, deriving among others sentences (33) and (34):

(33) The guard was killed by the ex-convict.
(34) It is the guard whom the ex-convict killed.

Intonation, the most flexible of all TD's, can shift the focus to

any desired element of surface structure. This is not to say that the suprasegmental TD's are merely superficial and have no effect on the meaning of the sentence. For, by simply shifting the stress from one word to the next, we can modify the underlying presuppositions considerably, as may be illustrated with the following two sentences, where the only surface difference is the stress pattern:

(35) Harry told me that Ann would be here.
(36) Harry told me that Ann would be here.

To the contrary, the intonation and stress pattern is greatly dependent on the underlying propositional focus which the speaker tries to communicate. (This fact, of course, has been known for a long time and need not be discussed here.)

The following sentence from German illustrates the interaction of lexical topic and suprasegmental TD:

(37) Die alte Frau sieht das Mädchen.
'The old woman does the girl see'.

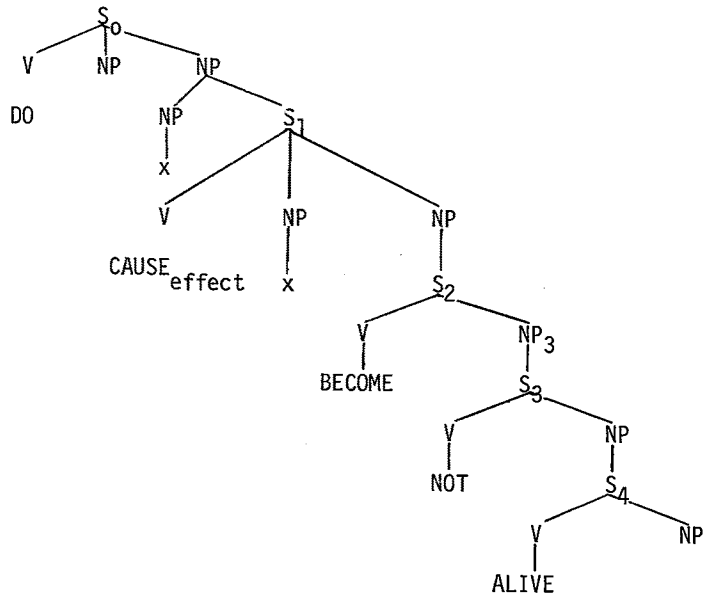
The verb sehen 'to see' recommends das Mädchen 'the girl' as topic. However, the speaker can disregard that recommendation and reverse the order of the two NP's, but keep their semantic functions constant with the help of stress. The refocusing is clearly done with reference to the lexical topic of the verb, which is responsible for the tension created in the surface sentence.

5. Focus selection as argument against lexical decomposition: If TD's of the grammar are hierarchically arranged as shown, with the lexical topic serving as reference point for focus selection, it follows that lexical items must play more than a marginal role in grammatical descriptions. The recent emphasis on relating as many 'paraphrases' as possible to a single underlying semantic structure has led many linguists to decompose lexical items freely into their presumed 'primitive conceptual elements'. However, the entire proposal of lexical decomposition, I believe, has resulted from a misunderstanding of the nature and function of lexical items. It is another instance of the problem Bolinger (1975) describes when he states that 'aggressive younger doctrines gained hold and shamelessly mixed all the levels into one stew' (p. 519).

I want to plead here once more for the integrity of whole lexical items. The lexicon may be tentatively defined as the linguistic breakdown of the experienced universe into minimal conceptual units which the speaker manipulates in verbal communication.[10] Every lexical item is an encapsulation of a certain conceptual structure and content. The question is how much of the 'elementary' conceptual structure is present in the actual lexical item, which serves primarily as a shortcut to permit human beings to communicate rather complex conceptual structures, and to do this efficiently.

We could, for example, say that the lexical item kill encapsulates the conceptual structure as in (38), adapted from McCawley (1972):

(38)



We could moreover argue that, when a speaker uses a sentence with the verb kill, he starts with the semantic representation as in (38) and replaces a portion of the tree structure at the appropriate point in the derivation with the lexical form kill. On the other hand, we may simply consider the verb kill a two-place action predicate that carries with it the recommendation that the agent be realized as sentence topic and the patient as direct object.

The generative semanticist is, of course, ready to defend the first alternative by pointing out that the almost in sentence (39) has three possible scopes. (There is a hint in McCawley (1972) that the sentence may be four ways ambiguous; but since McCawley is unable to decide between the two possibilities, I will not worry about that detail here.)

(39) John almost killed Fred.

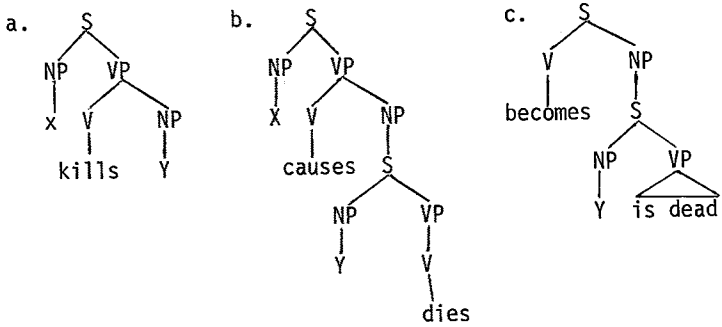
The three possible scopes of (39) can be illustrated with the following three paraphrases:

(40) a. John almost did something that would have killed Fred.
b. John did something that came close to causing Fred to die.

c. John did something that brought Fred close to death.

While the second part of the argument is true, it does not follow that the first part is also true. The native speaker is, of course, aware that (40 a-c) are paraphrases of (39) in specific contexts. In fact, he presumably knows that he can paraphrase causative verbs by using one of a small set of predicators (e.g., cause and make) plus the corresponding verb from the inchoative class; similarly he can take one of a small set of inchoative predicators plus the corresponding verb or adjective entry from the stative class to paraphrase an inchoative predicate, as illustrated in (41):

(41)



If certain scope relationships must be clarified, he can easily switch from the causative verb form (41a) to the decomposed alternative (41b). But the question is: Does he derive (41a) from something as complex as (38)?

I believe a number of reasons suggest that he does not.

i. Native speakers can, of course, be made aware that sentence (38) is at least three ways ambiguous. But it takes frequently considerable mental exercise, even among rather sophisticated language users, before they will notice all the scope possibilities linguists claim are present. Less sophisticated speakers will usually simply disregard the ambiguity problem altogether. If they are supplied with an appropriate context, however, they too have no difficulty selecting the required scope reading.

In other words, we have here clearly a case of indefiniteness rather than ambiguity, to use Weinreich's (1966) distinction. An analogy will help clarify the distinction. If I use the sentence

(42) There are ten balls.

every native speaker of English will presumably see the same number of balls. But it would be odd to say that the sentence is ambiguous because the balls could have been subdivided in different ways, e.g. in groups of five or in groups of two. This information is simply not specified. By the same token, we could say that the sentence

(43) There is a car in my driveway.

is ambiguous because the car may have three or four wheels or it may be gasoline- or steam-powered. Words are as efficient as they are because we can often use the same word to describe a wide range of similar objects and events, even though the details of the total conceptual situations vary.

ii. Further support, I believe, can be derived from the area of language acquisition. While I have not studied this question systematically, cursory observations of young children lead me to believe that children learn to understand and use causative verbs like kill long before they can handle their decomposed paraphrases. In fact, one six-year-old girl had no difficulty with sentence (39) and could even perceive the scope possibilities when the sentence was put into an appropriate context, but she appeared not to realize what was going on when she was given the corresponding scope paraphrases (40 a-c).

I have noticed a similar occurrence with foreign language learners. It is not uncommon to find non-native speakers of English who seem to have no difficulty understanding the meaning of causative verbs and who can easily recognize that the sentences (40 a-c) are paraphrases of (39) but who find it extremely difficult to detect scope ambiguities of negatives and adverbs in sentences with unit lexical items such as (39).

I believe that both of these facts point to the same conclusion: while mature English speakers will recognize the paraphrase relationship between, say, sentences (39) and (40a), the proper use of one does not necessarily presuppose a perfect understanding of the other. Children appear to understand the meaning of causatives like kill earlier than their decomposed paraphrases, because two-term predicates are cognitively easier to process.

iii. These facts can easily be explained, if we recognize that unit lexical items have a rightful place in the grammar apart from the conceptual tree structures they supposedly replace. They represent frequently a considerable shortcut, permitting the speaker to convert complex conceptual structures like (39) into simple propositions with two-term predicates. Speakers can, and do, use lexical items truly as shortcuts without relating them to the most detailed conceptual elements which linguists are able to conjure up for them. Because lexical items are shortcuts, linguistic communication is so efficient and language acquisition is greatly simplified.

Causative verbs exist, I believe, to make our thinking and communication efficient. They provide the speaker and the hearer with a shortcut for rather complex propositions. Yet, if the situation should require it, the speaker and the hearer can without difficulty convert the two-place predicate back into its conceptual elements. This is no different from what we do in other areas where cognitive processes are involved. For example, if someone were to ask me to multiply nine times eight, I would almost

automatically respond with 'seventy two'. But that does not mean that I could not have also proceeded algorithmicly, by taking one set of eight at a time and going through a nine-step operation to arrive at the result.

If lexical items reduce the complexity of underlying conceptual structures, why do languages also permit the decomposition of unit lexical items into syntactic constructions that mirror the conceptual structures either wholly or partially? I believe that there are two counteracting forces operating in human languages: one is the use of shortcuts via lexical items, resulting in a loss of conceptual transparency; the other is the tendency to preserve a certain degree of conceptual transparency.

It is this second force, I believe, that is partly responsible for the presence of constructions such as put under pressure, set in motion, and bring into focus even though near-synonymous unit lexical items may exist. We must ask, therefore, why conceptual transparency should be important in language.

I would like to suggest that the two major directions in GT grammar have failed to provide an answer to this question. Chomsky, because of his extreme position on the innateness controversy, has tried to give an explanatory account of the syntactic processes by relating them to innate properties of the human mind. He has, therefore, failed to realize that syntactic processes are functional in the communications process and that the major transformations have probably arisen to supplement the lexicon rather than the other way around. I believe that data from language acquisition support my view.

The generative semanticists, on the other hand, have fallen victim to the X=Y syndrome. In their attempt to relate as many 'paraphrases' as possible to the same semantic representation (a constraint forced upon them by their model), they have pointed out some interesting facts about conceptual structures, but they have not explained the paraphrases. The basic approach is always the same: in cases where the lexical item is simply indefinite as to certain conceptual details, they have extracted as many distinctions as can be contextually documented and attributed them to the lexical item itself. The result is that the conceptual structures that underly lexical items are constantly growing, with more levels being added to account for ever more contextually determined ambiguities. Where all this will end, only time will tell.

One of the most important factors responsible for the retention of conceptual transparency in surface forms is focus selection.[11] Propositions rarely occur in the same position in a discourse, and the distribution of old and new information in any given proposition changes constantly. The speaker must be able to focus on any element of the conceptual situation. Moreover, adverbial modifiers can have different elements of the conceptual situation in its scope. To accomplish this, the grammar must permit speakers to surface the same proposition with different focus options. Since the lexicon provides only for a

limited number of topic alternatives, as we have seen, the great bulk of the focus possibilities will have to be realized with the help of syntactic and suprasegmental processes. The major syntactic processes are then not only not autonomous, but, in fact, they merely supplement the lexicon in providing the different focus possibilities a given discourse might demand. The order in which the child internalizes the grammar of his language would seem to point to the same conclusion.

6. Syntactic consequences of focus selection: When the speaker selects a propositional focus, he factors out an element of the propositional structure and marks it in some way. The most important consequence is seen in the basic topic/comment division of the proposition, most naturally reflected in the subject/predicate division of the surface sentence, which is in turn determined by the recommended lexical topic of the predicate. Notice, however, that the syntactic organization of English sentences into subject and predicate does not merely reflect the topic/comment division; rather, the subject/predicate organization is so rigid that it, in fact, commits the speaker to select a propositional topic.

The speaker of English can, of course, focus on an entire proposition through the process known as nominalization. But there is an interesting use of the expletive there that permits the speaker to express a proposition without having to commit himself as to which element is the sentence topic. Consider the following sentences:

(44) a. There is a unicorn in the graden.
b. A unicorn is in the garden.
c. In the garden is a unicorn.

Sentences (44b) and (c) reflect the basic subject/predicate structure of English sentences. In both cases the speaker has to single out one element and elevate it into the position of surface subject. In sentence (44a), on the other hand, the speaker does not have to commit himself in this respect but lumps the entire proposition into the predicate phrase. Notice, incidentally, that this explains why the NP following there is has to be indefinite, i.e. why sentence (45) is ungrammatical with an expletive reading:

(45) *There is the unicorn in the garden.

The definite article is primarily used to refer to a known object or group of objects or to nouns that are unique (e.g., the sun). Since the topic is usually old information, definite NP's are good candidates for sentence topic. There is, thus, no reason why the speaker should select (45) rather than the expected.

(46) The unicorn is in the garden.

If the first NP is indefinite, however, it is usually new information and more likely to be incorporated into the comment. There-insertion thus serves to neutralize the overly strict requirement, resulting from the basic syntactic organization of English sentences, that every sentence must have a topic.[12]

The distinction between old and new information in propositions with one- and two-term predicates is easily accomplished within the subject-predicate structure of English sentences. But some predicates take three obligatory arguments, making a further distinction possible. The grammar of English must have devices capable of singling out one of several NP's occurring under VP and marking it as old information. The following sentences (to be spoken with normal intonation) illustrate this choice:

(47) Henry loaded the apples on the truck.
(48) Henry loaded the truck with apples.
(49) Penny gave her teacher the book.
(50) Penny gave the book to her teacher.

The underlined NP's in sentences (47)-(50) are usually felt to convey old information, while the last NP in each sentence introduces new information. Obviously, there are other differences between pairs of sentences such as (47) and (48), but they have been noted in many other places in the literature.[13] (A full discussion would also have to consider the effects of feature oppositions such as definiteness vs. indefiniteness.)

For lack of a better term, I will refer to the NP immediately following the verbs load and give in (47)-(50) as VP-topic. It defines that NP which is fronted by the passive transformation. Of interest to our study is the convergence of lexical and syntactic devices. The verb load has two lexicalizations, each with a different VP-topic recommendation. The verb give, on the other hand, has the same lexical VP-topic in both sentences: the direct object the book. The reversal is accomplished through a syntactic process that reverses the order of direct and indirect object.

7. Conclusion: I have argued that focus is not determined by any one of the grammatical components, but that lexical syntactic, and suprasegmental TD's interact to realize any focus option the discourse may require. TD's have been shown to be hierarchically structured, with the lexical topic inherent in the predicate serving as reference point for all focus selection.

The account of focus given here makes possible a unified description of the various paraphrase sets in English. It permits us to explain why there are so many alternative ways of symbolizing the same or similar conceptual structures without camouflaging that there are probably no two paraphrases in English with exactly the same meaning. The role of focus, moreover, helps clarify why many of the grammatical processes such as raising, passive, nominalization, tough movement, and extraposition should

be 'universal'. For, the same demand is placed on grammars of all human languages; every element of conceptual structure must be capable of being established as the center of attention. Rather than being a mere surface structure phenomenon, as Chomsky claims, focus is one of the major determinants of universal grammatical structure.

Footnotes

[1]I wish to thank John Calhoun, Vice-President of Texas A&M University, for providing travel funds to this conference.

[2]See Chomsky's footnote (21), where he states: 'The concept "normal intonation" is far from clear, but I will not try to explicate it here' (p. 89).

[3]Surface structure must not be taken literally. It is used here to separate the DS from all other steps in the derivation of a sentence. Thus it would be more accurate to say derived or surface structure.

[4]See also Derwing (1973:16ff.) for an extensive discussion of this point.

[5]See, for example, Hall (1973:31), Esau (1973:15), Bartsch and Vennemann (1972:6ff.), and Derwing (1973).

[6]A drill experiment in a linguistics or language class should easily convince anyone who doubts this.

[7]I fully agree with Chafe's arguments for the directionality of grammatical descriptions in Chafe (1970:56-61).

[8]See, for example, Fillmore (1968) and (1971).

[9]See my lexicon proposal in Esau (1974b) and also my discussion in Esau (1974c).

[10]See my discussion in Esau (1974b).

[11]It should be emphasized that focus selection is merely one of several factors. I am simply disregarding all other factors in this paper.

[12]One English instructor noted that he used the expletive there as a 'vocalized pause', an interesting confirmation of my analysis.

[13]See, for example, Fillmore's discussion in Fillmore (1971:386).

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PRAGMATIC METONYMY

Charles Ruhl
Old Dominion University

When a word acquires a new meaning, the new sense may have a referent which is contiguous to the referent of the previous sense; we have then an example of the semantic change known as METONYMY. An often-cited example of metonymy is the noun tongue, which designates not only a human organ but also a human capability in which that organ plays a conspicuous part. Another example is the shift of the word orange from the name of a fruit to the color of that fruit; since orange then refers to all instances of that color, we also have an example of the semantic change known as GENERALIZATION. A third example, cited by Bolinger (1971) in his response to Postal's (1970) analysis of remind, is the verb want, which once had the sense of 'lack' and now has changed to the contiguous sense of 'desire'. In these examples both senses survive, but previous senses have been lost for the verb win, whose earlier senses of 'desire' and 'strive' have been supplanted by the goal of the desiring and striving.

Metonymy is also a rhetorical figure of speech in which reference is made indirectly through something contiguous to the specified referent; the favorite example here is the use of knives to refer to 'warriors'. Adopting Richard's (1936, 96-7) terminology for metaphor and applying it in a parallel fashion, we call 'warriors' the TENOR of the metonym and knives the VEHICLE. Like all figures of speech, metonyms are often nonce forms, used for rhetorical effect; as in (1-3), they are quite noticeable, often deliberately so.

(1) You can even see a bitter cup of coffee on a man's face. (coffee advertisement)

(2) The road from Beverly to Salem is unusually busy this morning, and he [=Rev. Hale] has passed a hundred rumors that make him smile at the ignorance of the yeomanry in this most precise science [=witchcraft]. (Arthur Miller)

(3) The President [=Washington] took his reply out of his coat pocket. (William Maclay)

The metonym betrays itself by its collocational incongruity: in (1), it is not the bitter cup of coffee but the man's reaction to it that can be seen; in (2), Rev. Hale passes not the rumors but the people who relate the rumors; and in (3), Washington takes out not the reply but a piece of paper on which the reply is written.

However, not all metonyms are conspicuous. As (4-8) illustrate,

metonymy is also a useful device for saying something concisely when precision of reference is not necessary; the concise expression has a more dramatic, less verbal impact, and the incongruity is not immediately apparent.

(4) Most of the old roads have gone, and the motorcar, since then, has begun to cut the landscape to pieces. (Laurie Lee)

(5) All his policies, devious and opportunist as they had often been, had looked forward to the opening of her door to him [=Alexander the Great]. (Mary Renault)

(6) On Mount Washburn all of these safety suggestions were broken... (Jim Tallon)

(7) Holiday dinners fell, as a duty, on the lesser members of the organization. (Mary McCarthy)

(8) His second shot was batted out of bounds by Barry in a scuffle for the rebound. (David Dupree)

In (4), it is not the motorcar but the roads for the motorcar that cut the landscape; in (5), not the policies but Alexander, devising the policies, who looks forward; in (6), not the suggestions but the rules in the suggestions that are broken; and in (7), not the dinners but the preparation of the dinners that falls as a duty. In (8), we have a double metonymy: it is not on the shot itself but the rebound of the shot that the action occurs; and it is not the rebound but the ball that is batted out of bounds.

From these introductory remarks, we can suggest two roles for metonymy in a synchronic description of a language. We have SEMANTIC METONYMY for words like tongue and orange, in which the metonymic relationship is an explanation for the multiple sense, showing that the two senses have some aspects of meaning in common and that we thus have polysemic senses and not homonyms. By calling the variation semantic, we claim that it is LINGUISTIC knowledge, a property of lexical items. On the other hand, the nonce examples of (1-8) are instances of PRAGMATIC METONYMY, in which the multiple sense of the vehicle derives not from linguistic, lexical knowledge, but from WORLDLY knowledge, of how things are related in the world; in order for sentences (1- 8) to communicate successfully, the hearer must not only have linguistic knowledge, but recognize that the speaker intends something other than what he expresses, and that his indirection is based on contiguity. We can make this distinction between linguistic and worldly knowledge without implying any sharp division between them, but, on first impression, it appears that pragmatic metonymy will be limited only to nonce-expressions, and that more regular expressions will all be lexicalized.

There is also a third kind of metonymy, which we can call SYNTACTIC METONYMY. This results from elliptical processes, where one in John bought two books, and I bought one is a vehicle for 'one book'. Syntactic metonymy is an alternative explanation for metonymy in regular expressions, instead of polysemy, and is so used in some of the deletion rules of Transformational Grammar [hereafter:TG]. The distinction between syntactic and semantic metonymy is not discrete, with examples of the first becoming examples of the second, as in private (soldier), and borderline cases such as They're engaged (to be married), He was committed (to the hospital), He proposed (marriage), She's expecting (a baby), and I'll ask him on cross

(examination). The vague borderline can lead to debate over the best analysis of some expression; for example Green (1969) rejects a possible deletion solution to account for the short forms of (9) in favor of a solution that makes refuse polysemic.

(9) He refused (to grant) my request.
He refused (to accept) my offer.

Classical transformational theory assumed that linguistic knowledge was self-contained, internally complete and consistent, separate and independent of worldly knowledge; this assumption was necessary to assure that all the grammatical sentences of the language could be generated from a single axiom. This generative goal rests on the two crucial notions of COMPOSITION and DIRECT REFERENCE. If the meaning of a sentence derives compositionally from the sum of its syntactically ordered parts, and if all referential words refer directly and precisely to what they denote, then the sentence can be explained completely in syntactic and semantic terms. In other words, there is then no necessity for the Richards distinction between tenor and vehicle because these would always be the same. The correctness of composition and direct reference as assumptions appears self-evident; the apparant lack of any alternatives makes it difficult to see them as assumptions, just as it was difficult in physics to see that the notion of simultaneity was an assumption. With these assumptions, we make the linguistic system internally consistent and complete, and thus can equate natural language with artificial language. If, however, some sentences mean more or less than the sum of their parts, or if they refer indirectly either in part or whole, then we have evidence that natural language differs from artificial languages; as soon as there is a distinction between tenor and vehicle, the system is no longer internally consistent, and the goal of generativity becomes very difficult, if not impossible. As examples (1-8) show, we do have differences between tenor and vehicle; the transformational orientation had to assume that these were only nonce forms, tricks of performance, not significant data for the general patterns of the language. I will argue that pragmatic influence is in fact widespread in language, and that its presence derives from the experiential conditions of the human beings who use the language.

My argument has already been demonstrated in part by Bolinger in his answer to Postal. Bolinger suggests that at some point in the transition of want from 'lack' to 'desire' the latter sense was only INFERENTIAL, deriving from worldly knowledge, specifically the knowledge that someone lacking something is highly likely to have a desire to obtain it. Bolinger's general point is that any word can have such inferential senses; it is crucial not to assume that an inferential sense has been lexicalized, because we then "semantically overload" the lexical item (Bolinger's term), making the sense too specific. As a result of overloading, when a word suggests different things in different contexts, it will appear to be polysemic when it is not. This is no minor problem: it is the basic mistake of all semantic research, and is exemplified extensively in the excessive polysemy of the dictionaries.

Bolinger thus claims that some of the meaning communicated by any linguistic construction may be pragmatic; polysemy is often a

sign that pragmatic meaning is being misinterpreted as semantic. With this perspective, he criticizes Postal's analysis of remind and strike. Actually, his criticism does not involve metonymy but another type of semantic change and variation, that of SPECIALIZATION. Postal posits identical semantic structures for remind and strike as similar--an approach which makes remind and strike polysemic, since they are not always in synonymous constructions. As Bolinger demonstrates, remind need not involve a similarity, although a specific memory can, if the situation suggests it. Conversely, strike need not involve a memory, although in specific cases one can be struck by a memory. It is important to see that these specific cases are merely possibilities of more general concepts, not discrete and separate senses; the two expressions are not synonymous by semantics alone, but through PRAGMATIC SPECIALIZATION, in which sentences are interpreted more specifically than their semantics require. This is not a unique case; in fact, it is hard to find an example of a sentence in use which is not interpreted more specifically, in which some tenor is not more specific than its vehicle. Bolinger's insight is to enlarge the range of specialization, to give it a larger role in determining the meaning of sentences. In my opinion, PRAGMATIC SPECIALIZATION is the explanation of the specialized senses of time and causation in the conjunction and; it is also present in two of the most productive parts of the English language, compound nouns and verb particle constructions. Following Bolinger's initial comments on want, I will show that we also have widespread PRAGMATIC METONYMY.

Metonyms are not always nonce-forms; examples (10-13) (the latter two from Bolinger; 1975, 204) show that they also occur in regular constructions.

(10) Jobs are short in the country. (Robert Coles)
(11) She laid a fire, and he brought his flint and powder and lit it. (John Ehle)
(12) Turn the cereal off.
(13) The statute of limitations has expired on that bill.

In (10), it is not the jobs but the supply of available jobs which is short; in (11), the woman lays not the fire but materials for making the fire; in (12), not the cereal but the heat under the cereal is turned off; and in (13), not the statute of limitations but "the period of time during which according to the statute of limitations the bill was legally collectable has expired" (Bolinger; 1975, 204). The types represented by the metonyms in (7-8) are common as well; sports writing in particular is filled with sentences such as (8). These examples cannot be considered peripheral.

Metonymy has not been noted as such in TG, but when it has appeared in transformational examples, either the semantic or syntactic solution has been offered, as in (14-15), from Stockwell, et al. (1973, 721) [hereafter:SSP] and Borkin (1972), respectively [I've added the phone in (14)]. The collocational incongruity of (14) (consider such a response as What did the door ask?) leads SSP to assume that answer is a different verb from that of Nobody answered the question.

(14) Answer the door/the phone.
(15) Hanoi has refused to cooperate.

Answer is thus polysemic; this is the SEMANTIC solution. Borkin, on the other hand, proposes that Hanoi be a surface reflex of a deep structure approximating the government of the country whose capital city is Hanoi, on which a deletion rule, "head deletion," operates, deleting all but Hanoi, producing a "beheaded NP." The use of a deletion rule makes this a SYNTACTIC solution.

In accord with the notions of composition and direct reference, these are the only possible solutions [and were the only alternatives for Green in (9)], because these notions demand that the complete sense of each sentence be contributed BY LINGUISTIC MEANS. Both of the solutions produce complexity--many polysemic words, versus many deletion rules--but this is not sufficient criticism in itself; the complexity is in the relationship of form and meaning, and must be dealt with in some way. The problem with both solutions is that they lack insight and explanation, being merely bookkeeping devices; that is, they have the same inadequacy that TG found in Structuralism. The shortcoming is made evident by the fact that we cannot extend the solutions to similar phenomena without further complexity. Making answer polysemic in (14) requires that get be polysemic in a parallel fashion in (16). We also have (17), with dinner as in (7). In fact, the location of the problem is misunderstood in the SSP solution; it is not in the verb but in door.

(16) Get the door/the phone.
(17) I'll see to the door/the phone/dinner.

The role of door is the same in (14,16-17); it, along with phone, knock, bell, and buzzer all refer metonymically to acts of human communication in which they are instruments. Nor is a syntactic solution possible, since it too lacks generality: even with the similarities of door, knock, and buzzer, we have knock on/at the door, but no other combination, thus establishing three different patterns, and the need of three deletion rules for these three words. Of course, it is inherent in the notion of metonymy that some paraphrase can be found; but the cost of capturing the paraphrase relation is too high when deletion rules treat only highly specialized cases.

The simplest solution is to deny the assumption that all meaning derives from linguistic means, introduce the notion of metonymy into synchronic description, describe door as a pragmatic metonym, and assume the pragmatic rule that people can infer an act from an instrument used in that act. This is a complication for the complete grammar because we introduce a new notion; it also lowers the descriptive requirements of the grammar, because a pragmatic explanation does not, in principle, describe the meaning of a sentence as precisely as a semantic or syntactic solution would. But the loss of generality in semantic and syntactic solutions--a loss underestimated by transformationalists because analysis is still fairly limited--occurs because the requirements for linguistic means are too high. Some generalities involve worldly knowledge instead of linguistic knowledge, and cannot be stated in linguistic terms. We can see this with Borkin's example. The phenomenon she notes deals with encyclopedic knowledge, the capitals of countries; the pragmatic rule is that a speaker can use a capital city to stand for the government of the political entity--states as well as

countries--it is a capital of. To solve the problem non-pragmatically, the grammar would have to include all the information concerning capitals, and thus make the false claim that knowledge of capitals is linguistic knowledge.

This point becomes even clearer when we note that proper names can be used metonymically in much more varied ways; all the places listed in (18) have some distinctive event or situation attached to them, so that the name of the place can be a vehicle for the event or situation.

(18) Indianapolis, Nashville, Hiroshima, Little Big Horn, Kent State, Fort Knox, Chappaquiddick, Yalta, Pearl Harbor, Bethlehem, Watergate

To a lesser extent, with a more highly specified context, those of (19) can be used in the same way.

(19) Dallas, Memphis, Selma, Hastings, Versailles, New Hampshire

The difference between (18) and (19) is the difference between well-known information and more specialized or dated information; some of the names in (18), in fact, may be more like (19) for some people, or will become so in time. Since capitals continue to be capitals over time--but note Berlin--and are of more general interest than some items in (18-19), they appear to be more lexicalized; but their pragmatic character can be seen in their complete dependence on the basic tenor: if the capital of a country changes, then the country can be referred to only by the new capital name. This dependence is in contrast to real cases of polysemy, such as Mississippi and Missouri, where river and state, though related, could each be renamed without requiring a change in the other. These polysemic senses are in turn clearly distinguishable from names that have gone from pragmatic to polysemic to homonymic, such as china, limerick, and marathon. Names of universities such as Chicago and North Carolina are borderline cases; but when Wake Forest University moved from the town of Wake Forest, N. C. to Winston Salem, N. C., the name Wake Forest became clearly polysemic. Again, these distinctions are not fixed, and may be hard to resolve in some cases; but the case of Hanoi and other capitals is clearly pragmatic. Three more examples are (20-22); Arecibo in (22) refers to a radar and radio telescope.

(20) "She and the boys were with me in Japan. Then Korea broke, and I sent them back home." (Ben Haas)

(21) At Anzio, I saw a buddy of mine tied to a tree fifty yards from me, screaming for water, his face blistered in the sun. (Ken Kesey)

(22) Arecibo, because of its size and the quality of its receivers, can pick up extremely faint sources of radio waves. (James Gorman)

The generalization, in fact, is much wider than I have noted; these proper names exemplify the common metonymic pattern Container-for-Thing-Contained, which includes two more of Borkin's sentences (23-24). As (25) demonstrates, we have time examples too: the day is vehicle for 'a poor round of golf on that day'.

(23) This can [=the contents of this can] is contaminated.

(24) The whole apartment house [=all of the people in the apartment house] has hepatitis.

(25) The day took nothing away from 65 tournament victories, including nine major championships. (Denne H. Freeman)

The object of from is also metonymic.

Borkin's examples cover a wide range of types; she has several examples in which names of people refer to some attribute, possession, or production of that person (26-28). As (29-32) demonstrate, the vehicle can have varied tenors.

(26) Sam [=Sam's body] weighs 250 pounds.
(27) I'm [=my car] parked in a no-parking zone.
(28) Chomsky [=Chomsky's writings] is too complicated for freshmen to read.
(29) ... Gatsby informed Cody that a wind might catch him and break him [=his boat] up in half an hour. (F. Scott Fitzgerald)
(30) Vilas needed only 29 minutes to take the second set, breaking Nastase [=Nastase's service] in the third and fifth games...(AP)
(31) Mary got up and turned around so that Ma could unbutton her [=her dress]. (Laura Ingalls Wilder)
(32) Alice [=Alice's attention] was brought abruptly back to the table by Robin's voice. (Nancy Davis)

Some tenors are even hard to paraphrase, as in (32). The phenomenon extends to other noun phrases as well; Borkin supplies (33-34), and notes that "IBM can refer to an organization, a building, IBM stock or the price of it, various factions of the people on the IBM payroll, and conceivably even a bowling team..." (p. 42)

(33) Turn up the hi-fi [=the sound of the hi-fi].
(34) IBM [=IBM stock] is greatly overpriced.

Other noun phrases are equally prolific; anyone who considers a non-pragmatic solution for these examples has to be ignorant of the many possibilities, or of the descriptive complexities involved. Consider also (35), a sentence-type discussed by Culicover (1972);

(35) One more can of beer and I'm leaving.

the sense can be 'if you have one more can of beer', 'after I have one more can of beer', and other possibilities. A polysemic solution is impossible, and Culicover argues persuasively against a syntactic solution, noting the pragmatic-metonymic generalization that "the NP represents an event involving the NP" (p. 205).

In addition to providing a more general explanation, the metomynic solution also provides insight into the conditions under which people must use the language. The use of door in (14) fills a cognitive need, because the person answering the door typically does not know who is communicating, only the instrument being used; since the instrument is the only perceived aspect of the experience, it is what is lexicalized. Human experience thus imposes certain properties on natural language, properties that are not needed for artificial language. Of course, there are alternatives in natural language: indefinites like someone or something can be used in situations of ignorance. But this makes for wordy and bloodless prose, exactly what results in scientific jargon, which approximates more closely the standards of precision and explicitness of artificial language. In the guise of being scientific, TG assumes that just such a precise and explicit language UNDERLIES natural language, thus IMPOSING a pattern on the data that creates complexity whenever unscientific conditions--by far more common-- obtain. Important

to note also is the constant problem of any writer, the clash between rhetorical unity and changing sense; Warburton and Prabhu (1972) demonstrate this clash dramatically in (36-41).

(36) After John had undergone a sex-change he became a nun.
(37) John tore the document to pieces and threw it into the fire.
(38) When the baby grows up, it will become the mayor of the city.
(39) When a single cell has multiplied into millions, it becomes visible as a small lump.
(40) John dreamt that he was a woman and that he gave birth to twins.
(41) After the iceberg melted, it flooded the harbour.

In each of these cases, there is a rhetorical need to maintain a vehicle (or its pronominal substitute) as a topic, while the tenor undergoes a change that makes the vehicle inappropriate. In some cases, it may be possible to choose a vehicle that transcends the change, or to change to a new vehicle, but these options lead to wordiness and disunity; the metonym solves the problem. For conciseness and dramatic directness, to express lack of knowledge and to maintain rhetorical unity--for all these practical reasons, which need never be considered in artificial languages, pragmatic metonymy MUST occur in natural language.

Ironically, the laudable goals of precision and explicitness, if applied incorrectly to linguistic data, can lead to imprecision and lack of generality. This is what deep structure and the over-extended criterion of paraphrase have produced in TG. Conversely, an approach that recognizes the imprecisions and practical necessities of human language, and allows for them in a linguistic description, possesses the tools needed to capture generalities hidden by a seemingly diverse set of data. Consider the case of the verb break. On the basis of different syntactic patterns for break the window and break the news, SSP conclude (p. 733) that the distinction is that of, respectively, a CONCRETE break occurring with "physical objects of a certain degree of rigidity" and an ABSTRACT break occurring with abstract objects. The distinction between concrete and abstract is important for a scientific account, and thus it is usually assumed as axiomatic. But the distinction seems irrelevant for many words, especially the common verbs in English, like break, which have both concrete and abstract senses; to make this distinction axiomatic is to make all these words polysemic--a startling loss of generality. There is in fact a distinction between break the window and break the news, but it is not concrete-abstract; considering only the limited instance of direct objects, break occurs with the type exemplified by (42-50) in which the object is the THING BROKEN, and with the type exemplified by (51-59), in which the direct object is what RESULTS from the breaking, the thing broken not being mentioned.

(42) ...a hailstorm that broke every window on the north side of the Palace... (Fawn M. Brodie)
(43) Then taking his right hand in hers, she slowly raised his arm, breaking the scar tissue... (James Michener)
(44) ...there were no windows on my side of the building; nothing whatsoever broke the line of the continuous gray back... (George P. Elliott)

(45) The first faint sound of gently flowing water broke the silence. (James Joyce)
(46) The effort of answering broke the rhythm of his rocking... (F. Scott Fitzgerald)
(47) ...[the bond dissociation energy] is the energy needed to break one specific bond in a molecule. (Milton K. Synder)
(48) The man in this picture jumped 45 feet and landed on cardboard boxes, staggered to break his fall. (New York Times)
(49) In Lonesome Valley it was hard to break a teaching tradition more than a century old. (Jesse Stuart)
(50) This enemy, and only this one, had always broken her father's composure. (Irving Wallace)
(51) ...the rump of a fat man who had broken a hole through the stuffing of a couch... (Norman Mailer)
(52) They forded rivers and creeks, often breaking their way through ice...(Carl Sandburg)
(53) ...a great inland sea at one time covered the whole area round about until finally it broke a passageway through the canyon... (Joseph Wood Krutch)
(54) Sudden lightning storms erupted and broke bolts over the ocean. (Daphne Athas)
(55) A horse, drawing a buggy, broke his hollow music on the asphalt. (James Agee)
(56) ...working to figure out how to read a watch, breaking a sweat. (Ken Kesey)
(57) And Montana papers broke the story that federal agents, working undercover, had fingered a trophy poaching ring... (James K. Morgan)
(58) He [=Ford] broke the program first in an evening TV speech... (Joseph Kraft)
(59) The rest [of the students] will snore in the aisles as always, make paper airplanes from my notes, break wind in reply to my questions. (John Barth)

The distinction resembles the distinction in Latin between AFFECTED OBJECT and EFFECTED OBJECT (Gildersleeve (1953, 208)). We have a minimal pair with break trail in (60-61).

(60) They brought the dogs down to the creek, to where he [=the fugitive] broke the trail. (Thomas Wolfe)
(61) ...they had to break their own trail, worked harder and made poorer time. (Jack London)

Note that break occurs with both concrete and abstract affected objects; I have selected from over 400 examples of affected objects in Ruhl (in preparation), which incidentally demonstrate that the concrete-abstract distinction is often ill-defined. The SSP analysis is a classic example of what Bolinger calls "semantic overloading": we have concrete senses that are not rigid, such as break string/thread/rope/bag/cobwebs/blood vessel/skin/blister. While the concrete-abstract distinction creates polysemy, the affected distinction shows that the same break occurs everywhere; we gain a powerful generalization.

The relationship between affected and effected is a metonymic one. We can say that break as well as verbs like cut and tear have a structurally ambiguous direct object position; the subject position

is ambiguous in the same way. Alternatively, we can say that the affected type is basic, with the effected type a common break metonym; this interpretation is supported by the fact that the effected type is more marginal, less easy to generalize except as effects. But the effected objects are not the only cases of metonymy in the direct object position of break. Consider (62-63), describing the swing, respectively, of a golf club and a baseball bat.

(62) The left arm remains fully extended [on the backswing] and the wrists begin to automatically "break" (a cocking motion)... (Golf Tips)

(63) "You broke your wrists here." He indicated a point two-thirds through the arc of the swing. "Break 'em here."... I swung, snapping my wrists almost at the start of the swing. (Roger Kahn)

In neither case are the BONES of the wrist broken (or snapped): what is broken is the straight LINE of arm and hand when the arms cock or bend. That is, wrist is vehicle for 'line of arm and hand'; without a metonymic analysis in these cases, break wrist would have to be an idiom, and a generalization would be lost. The notion of line occurs also in (64-65).

(64) Bonding [in bricklaying] is the staggering or overlapping of units to break the vertical joints according to a definite pattern. (Lawrence B. Anderson)

(65) He broke his gun in disgust and reloaded. (Robert Murphy)

In (64) bricks are laid so that the joints of adjacent rows are not aligned; the line of joints is broken. In (65), the gun is opened for reloading in a manner that breaks the straight line of stock and barrel. Obviously, to specify line in each of these cases would lead to wordiness; (44) would be a more effective sentence if line were omitted. There are also metonyms involving the notion of 'barrier' or 'bound' as in (66-67), and also a number of other types, such as 'unity' in break balls [in pool], break bulk [=to destroy the unity of some package or cargo], break a dinner set, and break a dollar bill.

(66) "Listen, I could study forever and never break C." (John Knowles)

(67) ...the Knicks couldn't break 90 in their first two games... (Bob Ryan)

The verb break presents other complications, but these brief observations show how the notion of pragmatic metonymy can lead to powerful generalizations. In my research on break, I show that it is in fact a single verb, and that the procedures deriving from TG are faulty in that they lead inexorably to a polysemic interpretation. As I argued in Ruhl (1974), the basic problem in TG is one inherent in the choice of artificial language as a model: it is the assumption that THE ENTIRE LANGUAGE OPERATES ON COMPLETELY RATIONAL PRINCIPLES. What would a single general meaning for break be? This is not a question that can be answered in rational terms: the meaning is subconscious, and cannot be transformed by rational means into a paraphrase. As clear evidence that we possess linguistic powers we cannot rationally describe, and that linguistic expression has devious means, the verb break shows us that language is not an isolated human capacity, that linguistic knowledge interacts with

other knowledge, and that consequently a successful linguist must have a deep and patient understanding of human beings and all their ways.

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STRUCTURE IN DISCOURSE

Brian Phillips
University of Illinois at Chicago Circle

I. Introduction

The goal of linguistic theory is to provide a grammar of language, that is, a finite means of describing an infinite set of utterances in a language. In the most general interpretation of this goal, utterance is taken as discourse, and a discourse is in a language if it has meaningful coherence for a native speaker of the language. This view is not shared by all linguists. Some take the sentence - effectively the clause - as the domain of study, and some explicate only the syntactality of the clause. We wish to demonstrate that it is valid for linguists to formally explain the coherence of discourse (here written discourse) rather than to settle for narrower goals. It will be seen that the mechanisms necessary to resolve accepted linguistic phenomena, for example, determining the referent of proforms, are similar to those used to discourse analysis. Hence it is irrational to exclude discourse from the domain of linguistic study. We will also endeavour to show that the structure of discourse is tractible, it has an integrated structure, that is, a few rules of arrangement (for example, transformations) of a few items (for example, function words and function morphemes) and types of items (syntactic categories).

Although Bloomfield (1933) observed that 'Whatever practical connections there may be . . . there is no grammatical arrangement uniting (sentences) into one larger form.' (p. 140), there have been attempts to extend syntax to describe discourse structure. Harris (1964) uses a structuralist methodology to describe discourse. Sentences which have like distributions are collected into one class. When the environment of two elements is similar but not identical, chains of equivalences are set up to connect the two environments. This approach does not address the problem raised by

(1) John eats garlic. Nancy avoids him.

where we need to infer a casual link between the two propositions to understand the unity. On the other hand, (2) exhibits no such unity,

(2) John eats cheesecake. Nancy avoids him.

We must recognize the extensive use of implicature (Grice, 1975) in discourse. Implicatures are inferences that the speaker intends that the hearer makes an essential part of the message; (1) demonstrates this.

Some of these phenomena may be handled in syntax:

> . . . a deletion operation can eliminate only a dummy element, or a formative explicitly mentioned in the structure index (for example, <u>you</u> in imperatives), or the designated representative of a category (for example, the <u>wh</u>-question transformations that delete Noun

> phrases are in fact limited to indefinite pronouns . . .) or an element that is otherwise represented in the sentence in a fixed position.
>
> Chomsky, 1965, pp. 144-45.

But no current theory of syntax can explicate (1)

The domain of syntax is essentially the sentence. We should not expect that the structure of discourse to be of the same kind as sentential or clausal structure. Discourse is a unit because of its meaning, not because of its form.

If discourse is cohesive by virtue of meaning relations, a theory of discourse will depend on a theory of meaning. Moreover, the theory of meaning must include mechanisms to handle implicatures.

The difficulty in formalizing meaning is the principal reason why linguists have neglected discourse. Bloomfield (1933), and Katz and Fodor (1964) all felt that it is impossible to have a complete theory of meaning as it entails representing all the experiences of a speaker.

All of world knowledge is indisputably vast, but the principles by which it is organized may be quite restricted. The present endeavour is to seek the principles, and avoid being overwhelmed by the content. Or as Ruhl (1972) puts it

> The problem of accounting for 'all the knowledge in the world', raised by Katz and Fodor, can be resolved if we approach it in the same way an analogous problem is considered in syntax: accounting for the infinite situations -- analogously sentences -- with finite means.
>
> (p. 22)

II. World Knowledge

The model of world knowledge that is a necessary part of an explanatory model of discourse analysis will henceforth be termed an encyclopedia. Encyclopedic knowledge is brought to bear in going to a supermarket, planning a route to a cinema, choosing a TV program, driving a car, eating in a restaurant, etc. As such it underlies many aspects of behaviour in addition to its linguistic use.

An encyclopedia could be presented as a set of strings in surface form. This is not the most perspicuous means of representing knowledge for exactly the same reasons that surface form is not the best level for explicating the content of utterances.

Numerous models for encyclopediae have been proposed: Quillan (1965), Shapiro (1968, 1971), Simmons (1970), Schank (1972), Klein (1972), Winograd (1971), Hays (1973), Rumelhart, Lindsay and Norman (1972), and Wilks (1975). The canonical structures of our encyclopedia, based on Hays (1973), are given briefly below.

The model is presented as a directed graph. Nodes are labelled by concepts and arcs by relations between concepts.

There are four kinds of nodes: entity, event, attribute, and modality. The first three are cognitive counterparts of simple noun, simple verb and simple modifier, respectively. Modality nodes are in the contextual structure of cognition. It derives from Modal in the modal/proposition dichotomy of Fillmore (1969). Later examples, where ▭ is a modality node, will clarify the role of modality nodes in the encyclopedia.

There are four different kinds of relational organization in the

encyclopedia: Paradigmatic, Syntagmatic, Discursive and Metalingual.
PARADIGMATIC ORGANIZATION. Concepts exhibit hierarchic structure. The highest levels are taxonomy of Varietal concepts; intermediate levels are specific Instances of concepts; and the lowest levels are spatiotemporally distinguished Manifestations of concepts.

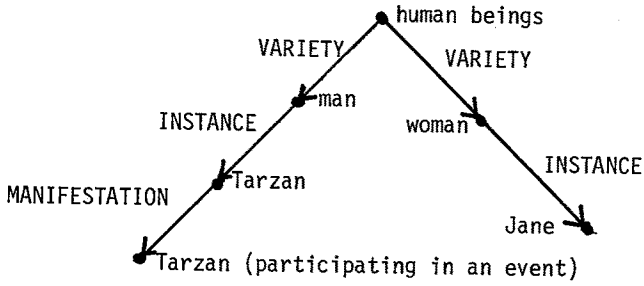

Figure 1

There is another Paradigmatic relation, Typical, used in the representation of concepts to which are attached noteworthy, but not universal, properties.

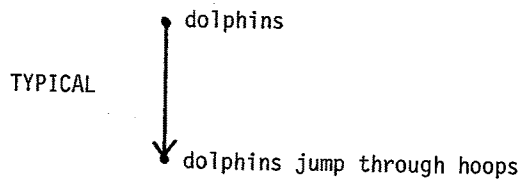

Figure 2

Paradigmatic structures also exist for events and attributes.

The prime function of paradigmatic structure is to give parsimonious representation for properties. For example, the properties of human beings that pertain to Tarzan and Jane need not be present at every node in figure 1. Instead they need only be associated with the one node 'human being'. Lower nodes only explicitly have their idiosyncratic properties, general properties are inherited from parent nodes.

SYNTAGMATIC ORGANIZATION. Syntagmatic structure characterizes the participation of entities in events, that is, the notions of Agent, Instrument, Patient and Objective, following Fillmore (1968). An event and its participants constitute a proposition.

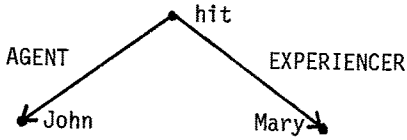

Figure 3

Other syntagmatic relations are Application (between an Attribute and an Event or an Entity), and Part-whole.
DISCURSIVE ORGANIZATION is formed by spatio-temporal and causal links between propositions. These relations actually link the modalities of propositions.

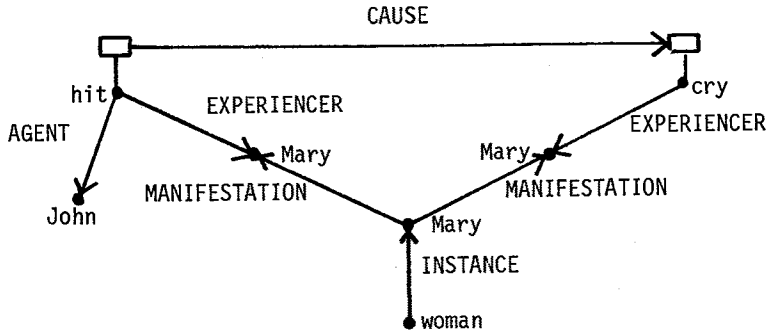

Figure 4

The coreference of the nodes labelled 'Mary' is indicated by their both being Manifestations of the same Instance, in figure 4.

Finally, we have METALINGUAL ORGANIZATION. People have knowledge about the meaning of abstract concepts, e.g., 'poison'. A general definition of poison can be stated and represented in the encyclopedia, with a pointer which indicates that if we find a story that matches the general story, then we have found an example of the abstract concept. So we define 'poison' as "A person ingests something which makes them ill". If our discourse contains "John ate some fish that made him vomit", it can be shown to match the general story, i.e., it has the same configuration of discursive and syntagmatic structure, and the specific concepts 'John', 'eat', etc., are paradigmatically related to their general counterparts, 'person', 'ingest', etc., respectively.

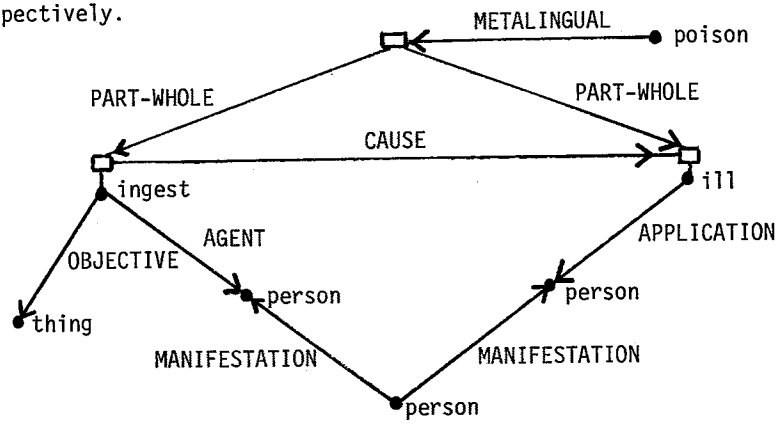

Figure 5

Note the conjoining of propositions in the definiens: a single modality node represents a context which has two parts. The dominating modality is linked by PART-WHOLE relations to the modalities of the conjoined propositions.

The process of replacing a story by metalingually defined concept is termed ABSTRACTION; the conversion process is DECOMPOSITION.

There is a methodological reason for the detailed presentation of the structure of our encyclopedia. Language is a very complex system. Hypotheses about linguistic structure also tend to be complex. This raises the problem of adequately and rigourously testing such hypotheses. The computer is an ideal tool for evaluating proposed solutions. In computer implementations the model might be spelled out precisely - surely a desirable point. Our encyclopedia is part of a computational system to test the coherence of discourse. However in this brief exposé, only minimal detail will be given; those wishing more are referred to Phillips (1975).

In addition to this study of discourse structure, the model has been incorporated into a computational model of car driving by a robot (Furugori, 1974), and non-computational descriptions of the belief system of a commune (White, 1975) and of literary analysis (Benzon, 1975).

III. Structure in Coherent Discourse

None of the current attempts to construct systems for discourse analysis has achieved widespread favour. This, we believe, is because they are not sufficiently perspicuous to show an integrated structure for discourse and they have not taken full account of the role of an encyclopedia. For a survey of contemporary studies of discourse structure, see Phillips (1975), or individual papers by Waterhouse (1963), Grimes (1972), Longacre (1968, 1972), Propp (1968), and Lakoff (1972). We will see that discourse expresses all types of cognitive organization: Paradigmatic, Syntagmatic, Discursive and Metalingual.

We postualte that the underlying form of discourse is a connected structure of propositions characterizing a logical sequence starting from facts that the author believes are known by his reader, and leading to his goal intent. The connections are furnished through Paradigmatic, Discursive and Metalingual structure. (Syntagmatic structure creates propositions, but does not connect them.)

To explicate the problems in discourse analysis, let us consider part of the process of producing a surface form of discourse from the logical sequence of propositions. Initially, propositions flagged for output are in the encyclopedia and are therefore connected to the rest of knowledge. Obviously, not all of the structure linked to the propositions can be expressed, else any discourse would contain the whole encyclopedia: that part containing the propositions of the intended discourse, but without connections to the whole of the encyclopedia. The problem is where exactly the cutoff should be. The principle governing the exclusion of encyclopedic structure from the copy is that deletions should be recoverable, i.e., the decision is based on a conceptualization by the author of the knowledge of the reader.

Understanding discourse involves reconstructing the omitted encyclopedic information. However, the sheer quantity of information in the encyclopedia enables long and complex inferences to be made to

connect even the most apparently disjoint sentences. Given

(3) Sonora Fnurd was a "Playmate of the Month" last year. She lives in Lackawanna.

as two unrelated incidents in the life of 'Sonora', an informant constructed a story to link them that involved thoughts about the type of woman that ends up in Playboy's centerfold, and an opinion about the habits of steelworkers, Lackawanna being dominated by a large steel company. Avoiding such pathological cases, we will try to see if there is a norm for what can and cannot be deleted, i.e., find constraints on the extend of reconstructive inferences, such that if too much is deleted, the discourse is incoherent.

CONNECTEDNESS

Paradigmatic Connectedness

Informally, we might say that a discourse is paradigmatically coherent, if we can infer that some if its constituents 'have something in common'. Formally, this means that they should have a common superordinate in an encyclopedic paradigm.

Is the coherence condition simply that a superordinate is inferred to exist, or is the constraint one of cognitive proximity of a superordinate to the superordinate concepts?

It may be the case that any pair of concepts has a superordinate and that we have to examine proximity to find constraints. If each node type has a paradigm that is single-rooted, then this root is a superordinate to all nodes of this type that appear in a discourse. For example, in figure 6, nodes A and B of an entity paradigm are linked to the general superordinate node, 'thing', of the paradigm.

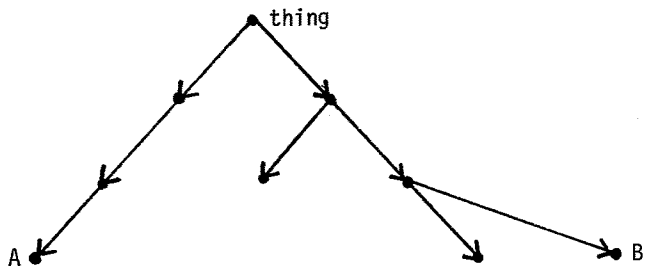

Figure 6

The universal existence of this node means that the existence of any superordinate cannot be used to state the constraint on the paradigmatic structure of discourse; a more restricted condition has to be found. Consider the following examples:

(4) Lions are indigenous to Africa. Tigers have stripes.

(5) John Smith is a congenital idiot. Mary Jones has blond hair.

(6) Flour is made from wheat. It is a principal ingredient in baking.

(7) Henry Kissinger married Nancy Maginnes in 1974. He wrote his doctoral thesis on Metternich.

All of these have common constituents with the paradigmatic form

Figure 7

where A represents an inferred superordinate of the subjects B and C of each example. The arcs AB and AC are both Variety in (4), Instance in (5), Typical in (6), and Manifestation in (7). In themselves, none of the examples are outstandingly good, but the last two are the more acceptable. These are examples of identity of 'sense' and identity of 'reference', respectively. Note that they are also structures in which pronominalization can take place.

A major problem with pronouns is finding the correct referents. There are syntactic restrictions on the likelihood of certain referents (see Langacker, 1969), but in

(8) John bought the wine, sat on a rock, and drank it.

both 'wine' and 'rock' are syntactically possible referents for 'it'. Why should we prefer 'wine' to 'rock'? The claim is that it is because the encyclopedia has a generalization of

(9) John drank wine

but not of

(10) John drank rock.

The generalization could be

(11) Animal inbibes liquid.

with each constituent paradigmatically related to a syntagmatically equivalent constituent in (11). The encyclopedia has not generalization for (10). That is a simple case to resolve. However,

(12) The town council refused a permit for the women to demonstrate as they {feared / advocated} violence.

is more complex. Depending on the choice of 'feared' or 'advocated', 'they' is usually taken as referring to 'town-council' or 'women' respectively. The decision is made on the basis of one's beliefs about the role of the town council in maintaining the peace, and a contradiction if a wrong choice is made.

Discursive Connectedness

In our encyclopedia, we have discursive relations to characterize spatial, temporal and causal relations between propositions. Thus we can analyse expressed discourse connectives, e.g., 'As a result' as a causal relation in the encyclopedia:

(13) As a result of the shock, she was struck dumb.

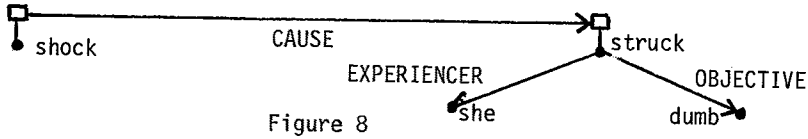

Figure 8

Or 'After' as the time relation Sequence in the network:

(14) The bomb exploded after Sean O'Toole left the pub.

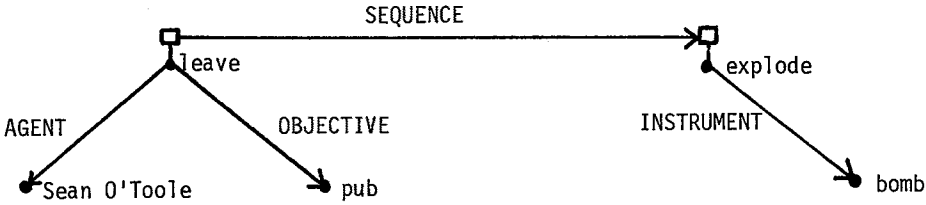

Figure 9

When expressed, a sequence marker can simply be mapped onto an appropriate cognitive structure, as in the above examples.

Discursive relations are frequently not expressed. Langacker (1969) notes that '. . . the order of conjoined constituents tends to be correlated with temporal order . . . of events . . . referred to.' (p. 171) This is true also of juxtaposed sentences. Compare

(15) John bought a new sports car. He showed it to all his friends.

and (16) John showed his new sports car to all his friends. He bought it.

The presentation in (16) is anomalous as it is counter to the order of happening. Similarly, causal connections may be omitted.

Consider the following news story from the New York Times about the Abominable Snowman.

(17) Katmandu, Nepal. January 5th.
The Abominable Snowman reportedly walked through snow near the tent of two American explorers in a remote area of Eastern Nepal on December 20th. Jeffrey McNeeley, a zoologist and leader of a six man team of young American scientists comprising the Arun Valley Wildlife Expedition, arrived here yesterday with three plaster casts of footprints that he believes were made by the legendary Snowman. The casts, he said, were made in six inches of fresh snow outside the tent of two fellow explorers at 12,000 feet. The Snowman, said to live in the upper reaches of the Himalaya Mountains, is thought by some to be an ape-like creature.

To understand this story, it is necessary to infer a causal link between 'Abominable Snowman walked through snow' and 'footprints'. The first step in the inference is to find general propositions for discourse propositions, e.g.,

(18) A living thing performs an act of motion in contact with something soft.

as in figure 10. This is done via the paradigmatic structure.

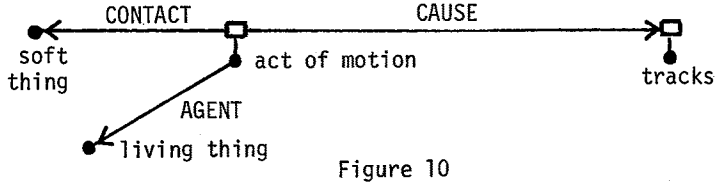

Figure 10

The discursive connections from the generalized propositions are followed until nodes are reached that have been activated from another part of the discourse, i.e., 'tracks', which is a generalization of 'footprints'. The encyclopedic discursive relations so found are added to the discourse, i.e., Cause in the present example, see figure 11.

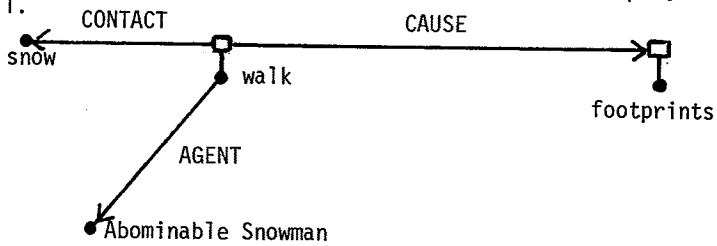

Figure 11

Currently exhaustive searches for discursive paths between nodes are made in the encyclopedia. Intuitively, there should be some restriction on the length of an inferred discursive path. For example, the following is a set of Causally related scenes:

(19) The sun rises. A cock crows. The farmer gets up.
. . .
The farmer's wife makes breakfast. The farmer eats breakfast. The farmer is warm and not hungry. The farmer is able to work. The farmer milks the cows.
. . .
John pours the milk onto his cornflakes. The cornflakes get soggy.

but a discourse composed of

(20) The sun rises. The cornflakes got soggy.

is of the same ilk as (3). In (21), there is a time sequence of widely separated scenes; it is not acceptable.

(21) Magna Carta was signed in 1215 at Runnymede. Columbus discovered America in 1492. Napoleon was defeated in 1812 by the Russians.

At present constraints on discursive connectedness are open problems, but one parameter of an eventual solution is likely to be the number of alternatives on an inferred connection. For example, if some propositions are arranged in the encyclopedia as in figure 12,

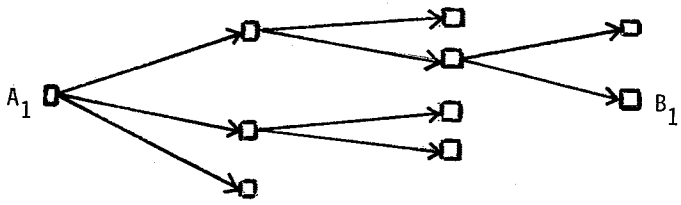

Figure 12

the arcs being any discursive arc, then the confidence in an inferred

path A_1B_1 must be reduced by the number of alternatives. If the structure was as in figure 13,

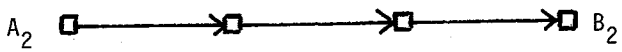

Figure 13

then A_2B_2, although containing the same number of arcs as A_1B_1, is presumably more reliable an inference.

THEMATICITY

The second necessary condition on coherent discourse is that it should have thematic structure, in fact a recursive thematic structure. Lacking any contrary evidence, we accept that the number of themes may be unlimited in the same way that the vocabulary of language is open.

Thematicity cannot be characterized by arcs between nodes. The nature of the relation is that a discourse has a theme relative to the encyclopedia if it matches, directly or indirectly, a generalized story that defines the theme. The formal statement of a theme is identical to that for metalingual terms, and the process for establishing a match is the process of Abstraction. In many cases, themes are indistinguishable from abstract terms. For example, 'charity' is an metalingual term that can also serve as a theme.

In this section we wish to justify the claim that a thematic structure condition is universal by examining different examples and analyses of general discourse for evidence.

A theme may be explicitly stated in discourse. In technical writing it is quite usual to express a complete definition, definiendum and definiens: Kuhn (1962) defines 'paradigm' as an 'achievement' that is '. . . sufficiently unprecedented to attract an enduring group of adherents away from competing scientific activity . . . (and) sufficiently open ended to leave all sorts of problems for the redefined group of practitioners to resolve.' (p. 10). Much of the rest of the book then discusses paradigms as models for scientific revolutions.

If a discourse has an implicit thematic structure, it has to be inferred by a reader. An author, therefore, must use themes that are known to the reader. There are studies which indicate the existence of abstract themes in language.

In fairy-tales, Propp (1968) analyses a reader's expectancies about the structure of the tale. Propp starts by comparing the following events from different tales:

1. A tsar gives an eagle to a hero. The eagle carries the hero away to another kingdom.

. . .

2. A princess gives Ivan a ring. Young men appearing from out of the ring carry Ivan into another kingdom. (pp. 19-20)

Propp infers that '. . . a tale often attributes identical actions to various personages. This makes possible the study of the tale <u>according to the functions of the dramatis personae</u>' (p. 20). Folk tales are analysed in terms of functions. The above examples are described as containing two functions: 'Acquisition of a magical agent' and 'Transference to a designated place'. An example of Propp's analysis is

(22)

ACTION	FUNCTION
A tsar, three daughters.	INITIAL SITUATION
The daughters go walking,	ABSENTATION
overstay in the garden.	VIOLATION
A dragon kidnaps them.	VILLAINY
A call for aid.	MEDIATION
Quest of the three heroes.	CONSENT TO COUNTERACTION DEPARTURE
Three battles with the dragon,	STRUGGLE VICTORY
rescue of the maidens	INITIAL MISFORTUNE LIQUIDATED
Return,	RETURN
wedding.	WEDDING

(p. 128)

Functions correspond to metalingually defined concepts of the encyclopedia. Propp shows that this genre of discourse can be analysed as an ordered string of abstract concepts.

Lakoff (1972) gives a transformational model for discourse that uses Propp's set of functions. A phrase structure component generates a 'deep structure'; for example, the tale of (22) may be represented by the hierachic structure of figure 14.

Linde (1974) finds that there is a prescribed pattern in verbal descriptions of apartments. Only two discourse strategies are used by her subjects to express the spatial structures, and of these, one is considerably more frequent than the other:

> There are at least two logical possibilities for . . . [the overall description of apartment layouts] . . . the speaker may describe a map of the apartment, or he may describe a tour of it. Examples of each are the following:
>
> > I'd say it's laid out in a huge square pattern, broken down into four units. If you were looking down at this apartment from a height, it would be like . . . like I said before, a huge square with two lines drawn through the center to make four smaller squares. Now on the ends . . . uh . . . in the two boxes facing out on the street you have the living room and a bedroom. In between these two boxes you have a bathroom. Now between the next two boxes, facing on the courtyard you have a small foyer and then two boxes, one of which is a bedroom and the other of which is a kitchen and then a small foyer a . . . a little beyond that.
>
> > Well you walk in the door and there's a kitchen and then off the kitchen is one bedroom. As you go straight in from the doorway through the kitchen you go into the living room. And then to the left of the living room are two bedrooms. The two bedrooms are on the same side of the building and the living room and the kitchen are on the same side of the building.

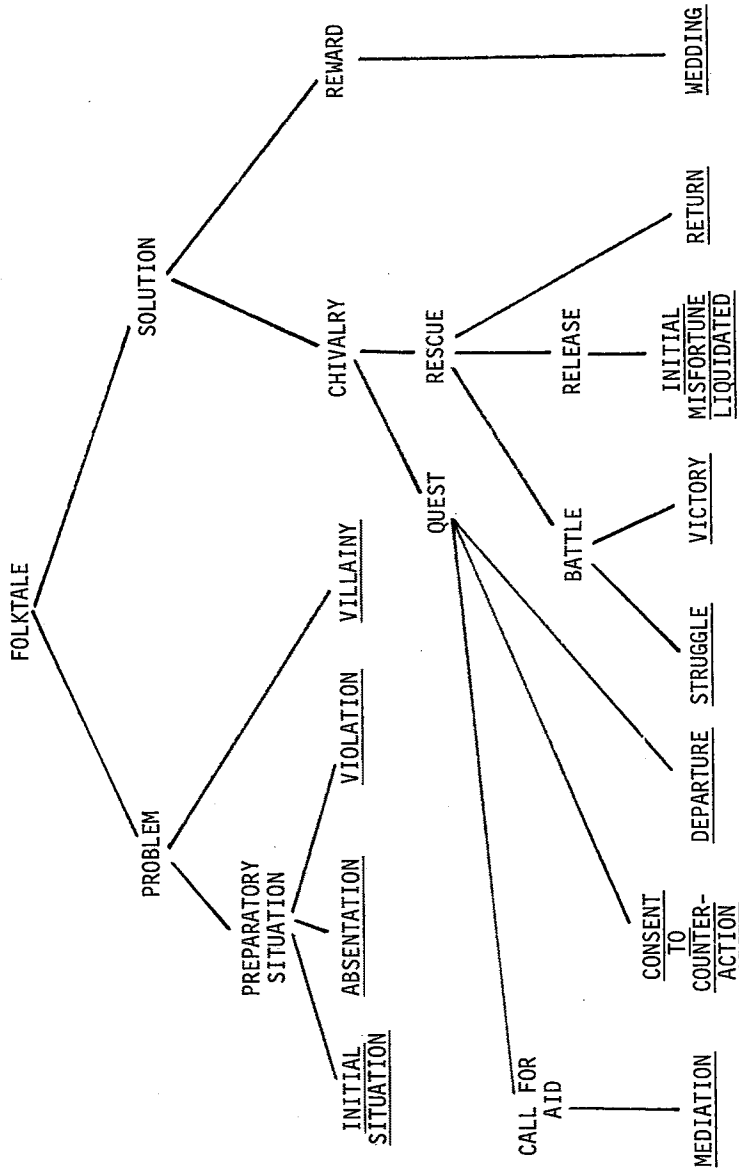
FOLKTALE
SOLUTION
PROBLEM
REWARD
CHIVALRY
VILLAINY
VIOLATION
ABSENTATION
INITIAL SITUATION
PREPARATORY SITUATION
RESCUE
QUEST
CALL FOR AID
RELEASE
BATTLE
WEDDING
RETURN
INITIAL MISFORTUNE LIQUIDATED
VICTORY
STRUGGLE
DEPARTURE
CONSENT TO COUNTER-ACTION
MEDIATION

Figure 14

> Both of these descriptions are reasonable answers to the question "Would you describe the layout of your apartment?" Our intuition certainly informs us that both speakers have fulfilled the task that was proposed them. What our intuitions do not tell us is that descriptions like [the first] are extremely rare, while descriptions like [the second] are extremely common. Of 72 apartment descriptions, only 3 are of the form of a map . . . while 69 are of the form of a tour . . . (pp. 8-9)

The theme that we suggest to characterize the commonest form of description is 'path', which may be thought of as a sequence of spatially localized propositions, and the parts of the path are in the locations of the initial and final locations, e.g., 'a path from New York to Washington'. A 'tour' is a sequence of paths.

Longacre (1968) notes that in a given language there is a finite number of discourse types which can never be mixed or confused. Discourse from various Philippine languages suggest 4 contrasting discourse prose genres:

Narrative: recounts some sort of story.
Procedural: tells how to do something.
Expository: any sort of explanatory essay.
Hortatory: attempts to influence or to change conduct.

Narrative discourse is composed of the following tagmemes:

±APERTURE ±EPISODE ±DENOUEMENT ±ANTI-DEMOUEMENT ±CLOSURE ±FINIS

APERTURE provides temporal and spatial setting and introduces some of the principal dramatis personae.
CLOSURE gives final commentary on main participants 'they lived happily ever after'.
Nuclear tagmemes EPISODE, DENOUEMENT, and ANTI-DENOUEMENT show a great variety of exponence . . . typically any paragraph type may be an exponent - plus embedded discourse of the PROCEDURAL or EXPOSITORY genre.

A correspondence can be informally recognized between some of Propp's functions and Longacre's tagmemes. For example, between 'Initial Situation' and 'Aperture'; and 'Reward' and 'Closure'. For Propp, the peak of a discourse is in the function 'The Initial Misfortune Liquidated', and for Longacre it is in the tagmeme 'Anti-denouement'.

We conclude that there are prescribed patterns in all genres of discourse; we term these patterns 'themes'. We do not have an inventory of themes; their discovery is a matter of empirical investigation.

Any extended discourse is unlikely to be organized according to a single theme, but will exhibit recursive thematic structure. We hypothesize that a coherent discourse is characterized by a single-rooted tree of themes, as schematized in figure 14.

All themes must be proper subthemes of the matrix theme. A text with an overlapping thematic structure is incoherent:

(23) Eating fish made John sick. He caught measles last May.

shown schematically in figure 15.

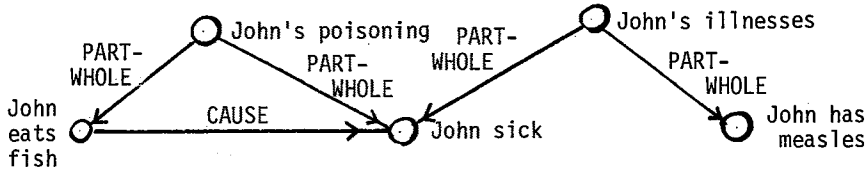

Figure 15

An important point to conclude this section. The inferred connections may not correspond with those intended by the writer. This is another problem. Here we only address the analysis of a story by a reader. If he connects in the manner described above, then it is coherent for him.

IV. Analysis of Some Stories

We want to show that abstract patterns are more general, that all linguistic behaviour is instances of patterns. Obviously such a claim must be substantiated by the discovery of such patterns.

To test the hypothesis, students were asked to write short stories about 'accidental drowning'. Elsewhere (Phillips, 1975) all of the stories are categorized and each category analysed. Here we will only examine two of them, two that are reasonably different expressions of the theme; further, the second exhibits theme embedding.

The instructions given sought only to define a topic and an approximate length; it was hoped to impose no further constraint on the content of the story: 'Write a drowning story that, for example, you might expect to find as a column filler in the New York Times'. An example:

(Story 1)
The body of Horatio Smith was found last night in the Niagara River. He was drowned when his boat overturned on the river.

The hypothesis formed is that an acceptable drowning story must give the following information:

(a) Why the victim was in the water.
(b) Why the victim was not able to save himself.

The rationales for these requisites are:

(c) A person is not usually found in water, and therefore some explanation of this location is expected.
(d) By an instinct of self-preservation, one would expect the victim to try to extricate himself from his predicament. The story should say why he couldn't.

One habit in discourse is to set the stage: 'Initial Situation' (Propp); and 'Aperture' (Longacre). In terms of our model this theme should be recognizable by the occurrence of space and time relations. Thus in story 2 below we have '. . . today . . .', . . . in Middle Branch Resevoir . . .', etc. A greater complexity of expression of thematic structure is to be expected elsewhere in the stories (see Longacre's comments on nuclear tagmemes, above). In our analysis we exclude the 'stage setting' theme and focus on the remainder. Further, taking an ethnocentric view of the world (cf White's teleological commune) we suggest that in their underlying form, the remaining part of our stories will have a causal structure, and only causal in-

ferences will be relevant in understanding.

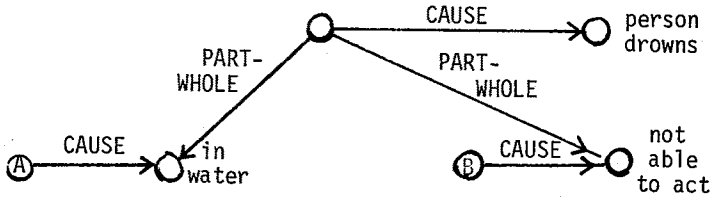

Figure 16

The empty nodes A and B in figure 16 indicate that the story proper must have something that stand in a causal relationship to the other propositions, i.e., explain why they happened (what caused them). If not explicit, this information must be recoverable from common knowledge. In story 1, none of this information is immediately present. However, the story is acceptable, so facts from common knowledge must be used to reconstruct a fuller form which conforms to the prescribed thematic pattern of figure 16. For the analysis of story 1, the following knowledge is used:

(i) If a person is in a boat and the boat overturns, this may cause him to be injured and in the water.
(ii) If injured, a person may not be able to 'act'.
(iii) If a person is in water and cannot 'act', then he may drown.

The antecedent conditions expressed in a general form in (i) match the specific situation in story 1. Thus we can infer that Horatio Smith is in the water and he is injured. From (ii) we can infer that he is not able to act. From the inferences that he is in the water and not able to act, we infer that he drowns, a fact stated explicitly in the story. We have successfully linked the propositions expressed in story 1, as shown in figure 17.

Further we have explanations for Horatio Smith being in the water: (the boat he was in overturned) and for him not being able to act (he was injured). The theme fits. We have a connected discourse with single rooted theme; it is coherent.

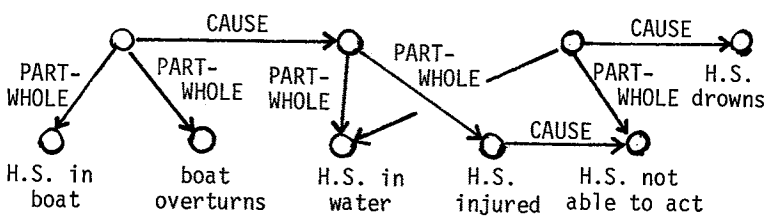

Figure 17

(Story 2)
Dino Frances, 43 years old, of Queens, drowned today in Middle Branch Resevoir after rescuing his son Dino, Jr., who had fallen in the water while on a fishing trip at Tilly Foster, near here, the police said.

Story 2, taken in fact from the New York Times, looks like a drowning story, and, as we shall show, does contain this theme. However, it contains more. The claim is made that it is a 'tragedy'. This theme is defined as a situation in which "someone does a 'good act' (e.g., rescue) and dies (e.g., drowns)", (iv) below.

Earlier, the encyclopedia contained a situation which could result in a person drowning (iii). An alternative consequence of the same situation is that someone may carry out a rescue (v). Also, if a person does something, it may cause him to get 'weary' (vi), and if 'weary', he may not be able to 'act', (vii). Another fact brought to bear in this story is that in order to rescue someone who is in water, it may be necessary for the rescuer to be in the water (viii). We also need to know about falls causing injury, (ix).

(iv) 'Tragedy' a person does a good act and dies.
(v) If a person is in water and cannot act, someone else may try to rescue him.
(vi) A person does an act for someone, that person can get weary.
(vii) If a person gets weary, he cannot act.
(viii) If a person wants to rescue someone who is in water then the person may have to be in the water, too.
(ix) If a person falls, he may be injured.

With this knowledge a connected series of propositions, figure 18, can be inferred.

Propositions A, B, C, D, E and F satisfy the 'accidental drowning' theme. The same propositions together with X satisfy the 'tragedy' subtheme. Although the story has two themes, 'accidental drowning' is a proper subtheme of 'tragedy', i.e., all the propositions in the former are also in the latter. By our earlier hypothesis this is an acceptable organization of themes, for a well formed discourse.

V. In Conclusion

We have presented a rudimentary system for discourse analysis. But note that we have talked only of underlying logical and thematic structures; we have not begun to treat the lexical and syntactic realisation of this underlying structure. For the former we are aware of no formal treatments; for the latter there are a number of automatic parsing systems under investigation: Winograd (1971); Heidorn (1972); Grishman, Sager, Raze and Bookchin (1973); Kaplan (1973); Kay (1973); and Woods (1973). None have yet reached a stage of development that could handle the aspects of discourse structure given here.

It is evident that many of the problems in discourse analysis have not been seriously considered by any current linguistic theories. In particular we pinpoint the neglect of inferences based on encyclopedic knowledge. As Wilks (1975) puts it '. . . [transformational grammar] was set up to be quite independent of all considerations of meaning, context and inference, which constitutes something of a disqualification for . . . understanding language.' (p. 264). And moreover

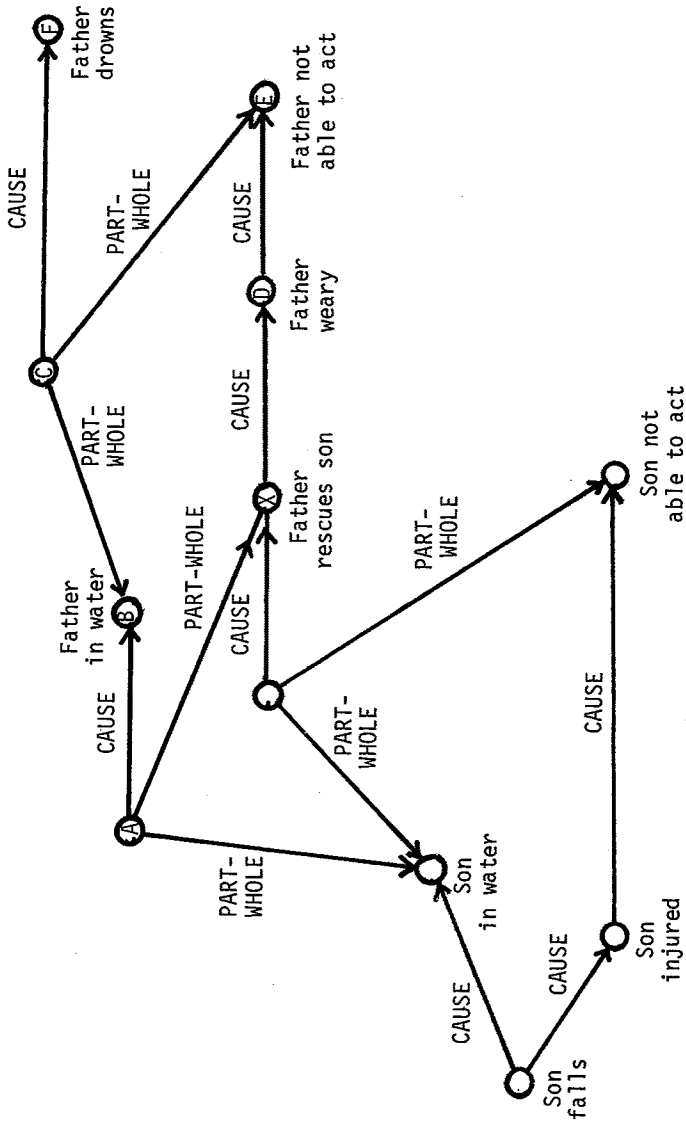
Father drowns
F
CAUSE
PART-WHOLE
Father not able to act
E
CAUSE
Father weary
D
C
PART-WHOLE
CAUSE
Father rescues son
X
Son not able to act
PART-WHOLE
Father in water
B
PART-WHOLE
CAUSE
PART-WHOLE
CAUSE
A
PART-WHOLE
Son in water
CAUSE
CAUSE
Son injured
Son falls

Figure 18

(Wilks, 1974) '. . . the generation of sentences, with whatever attached structures, is not in any interesting sense a demonstration of human understanding.' (p. 276).

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A COGNITIVE MODEL FOR THE SOCIAL CONSTRUCTION OF REALITY: PROGRESS REPORT*

The Second Foundation

Michigan State University

Where We're Coming From

The linguists of the forties and fifties neglected the concept of meaning with respect to the analysis of language; we believe that the linguists of the sixties and seventies have neglected the concept of culture with respect to the analysis of meaning.

In keeping with Yngve's proposed rehumanization of linguistics and Pike's search for a unified theory of human behavior, The Second Foundation has been investigating the relationships between social systems and linguistic systems in an attempt to discover and describe how society determines what is meaningful and what is not, how this socially determined meaningfulness is communicated through verbal and nonverbal behavior, and how knowledge of this activity might be applied in counseling-type relationships.

This paper is a preliminary report of our findings to date and some of our projections for future research. Although we work primarily within a stratificational/cognitive framework similar to that originally developed by Gleason and Lamb for describing linguistic behavior, we owe a great debt of gratitude to such thinkers as Pike, Longacre, Yngve, and Halliday in linguistics; Scheflen, Berne, Harris, and Jones in psychology; and Berger, Bock, and Williams in anthropology and sociology.

Central to our investigations has been the concept of role; in many ways, the sociological abstraction of roles parallels the linguistic abstraction of phonemes. In both instances observable behavior is related to a more abstract point in the structure of the system in question. In the case of the phoneme, such observable behavior is the acoustic manifestation of a language. In the case of the role, however, this observable behavior is much more complex, the realizations of a role taking both linguistic and kinesic forms.

Another important concept within our framework is that of <u>transaction</u>. Originally culled from transactional psychology, where "the transaction consists of a stimulus by one person and a response by another, which response in turn becomes a new stimulus for the other person to respond to" (Harris 1969:89), we have expanded the definition of transaction into a form which bears close resemblance to Pike's <u>behavioreme</u>, which he described as roughly <u>any</u> unit of or-

ganized human behavior within a behavioral system (or systems) which has verbal and/or nonverbal manifestations.

The next part of this paper is a discussion of the structure of a society, how roles arise within this structure, and how these roles relate to the society's roster of transactions and, ultimately, to linguistic/kinesic behavior.

Where We Are

One of the prerequisites for the study of sociolinguistic phenomena within a particular society is a suitable analysis of that society--the structure of families and groups, and larger, more formal units, if any; the types of roles which arise from these structures and when they are played--in short, everything which has classically been the province of anthropologists, sociologists, and their ilk. Unfortunately, as we are beginning to see, linguists cannot afford to ignore this aspect of human behavior when dealing with another; all too often we have been successfully describing parts of the iceberg which appear above water while forgetting those parts which are submerged!

In an earlier paper[1] we discussed the nature and "placement" of certain sociolinguistic phenomena within the linguistic system. Accepting the concept of a gnostemic stratal system--that is, a stratal system dealing with the nature and organization of a speaker's knowledge--we reasoned that the social constraints on language use and form must arise at this level, determining such things as textual genre, social accents, and perhaps even the patterns of conversations and the range of topics in them. All of these phenomena and more are presumably predetermined to a large extent for the speakers by their culture (Berger 1963:117).

In an effort to provide a more solid working base for the investigation of sociolinguistic phenomena within American industrialized society, The Second Foundation has been studying the structure of that society and how it relates to linguistic structures employed by its members. Our analysis has forced us to reject the notion of a single gnostemic stratum for the present; it may very well be that one such stratum will satisfactorily account for the behavior of an individual, but we believe that an adequate description of the principles which underlie this behavior requires a multi-stratal treatment.

Before continuing with a discussion of our findings, we feel that some additional comments on our position are in order.

First, this is not the first time that a multi-systemic, if not actually multi-stratal, configuration of social behavior has been posited. Scheflen (1972) discusses kinesic behavior with reference to two systems, one related to the popular conception of

"body language" as revealing the verbally unexpressed motivations and emotions of the individual, the other related to the maintenance and regulation of the social order within which the individual is functioning. In his article concerning language as social semiotic, Halliday gives a detailed look at some of the socially-defined systems which he believes interact with the linguistic system to produce an utterance. Yngve discusses human linguistics with relation to "groups" and "populations," two social divisions which he feels are necessary to recognize in order to adequately deal with human communication.

Second, although our view of a multi-stratal nature of higher systems of human behavior parallels in some sense the stratificational position that language is a series of relation-linked strata, it was not our initial goal to develop such a parallel. Indeed, our first attempts at formalizing the inter-relating social and linguistic systems were aimed at integrating our knowledge of both into the existing model of the gnostemic stratum. Furthermore, we admit the possibility that further work on our part or on the part of other investigators may achieve this goal.

Finally, we would like to emphasize that while not actually a part of the language system per se, these higher levels are intimately related to it, and simultaneously form both part of the individual's knowledge of his culture and part of the interface between his language and reality--albeit a largely socially-defined reality.

In our present theoretical framework, we have retained the "original" gnostemic stratum in the form of a transactional stratal system. The primary component of this system is the transaction tactics, which generates transactions in a way analogous to the generation of sentences by the lexotactics.

The transactional stratum essentially deals with human activity in its most widely varied forms. It is at this level of human behavior that the form of Lamb's baseball games and non-transformational dinner parties, Pike's church services, Ikegami's classical Indian dances, and Scheflen's kinesics of greetings are all specified. Secondary roles such as Di Pietro's Don Juan/Southern Belle examples may also belong to this level of behavior.

In addition, all the phenomena which are traditionally assigned to the gnostemic stratum are included within the transactional system: connections to memory and taxonomies, by which a speaker organizes, categorizes, and relates information according to both socially and individually defined criteria, and text-generating systems, which provide for conversations, formal and informal speeches, sonnets, Török Pasa-like didactic-performative rhymes, and all other types of linguistic behavior which may form a transaction in its own right or a part of transactions of higher rank

within the tactics.

Having explained the general form of the transactional stratum, we may now more closely examine its functions with relation to a higher stratal system which deals with the internal structure of society and the origin of roles within that society.

As previously stated, our analysis of American industrial society seems to require one stratal system above the transactional system, and it is likely that all human societies follow a similar pattern; the American system is unlike those of many societies in that it has two tactic systems operating on the same level. This bi-tactical treatment is necessitated by the different natures of behavior patterns in industrial society and the realization of its roles on the transactional stratum.

These two tactics--sociotactics--correspond in many ways to the classical sociological distinction of Gemeinschaft versus Gesellschaft. Whereas the relationships in a Gemeinschaft are organized around kinship groups, are collectivity-oriented, and share a "community of fate" in that it is felt that what happens to one member of the Gemeinschaft happens in some measure to all, Gesellschaft relationships are generally organized around work roles and are individualistic and impersonal.[2]

This social dichotomy is of course reflected in roles and the type and structure of transactions which realize these roles. Roles originating as determined elements within the sociotactics exercise control over the transactional stratum much in the same way that the sememic stratal system controls the output of systems beneath it.

For example, the lexotactics operating alone will produce grammatical nonsense of the sort "The sleeping table married its jumping lake"--grammatical in that it conforms to lexological, morphological, and phonological patterns of English, nonsensical in that our real-world experiences seldom provide us with the stimulus to produce such a sentence.

In the same way, the transactional stratum would generate a transaction in which a secretary tied a bib around her boss' neck and told him "Now clean up your plate!" This transaction is socially nonsensical according to what we know about secretaries and their employers, although it is well-formed as a transaction. If we change the dramatis personae and consider the roles of mother and child, however, the transaction is both well-formed and sensical.

A stylized diagram of these two stratal systems appears in Figure 1. It should be mentioned that this diagram is greatly simplified for ease of presentation; R_1, R_2, and so on represent roles, and /ego_1/ and /ego_2/ represent the individuals involved. Realizations of transactions on linguistic and kinesic levels are not indicated here.[3]

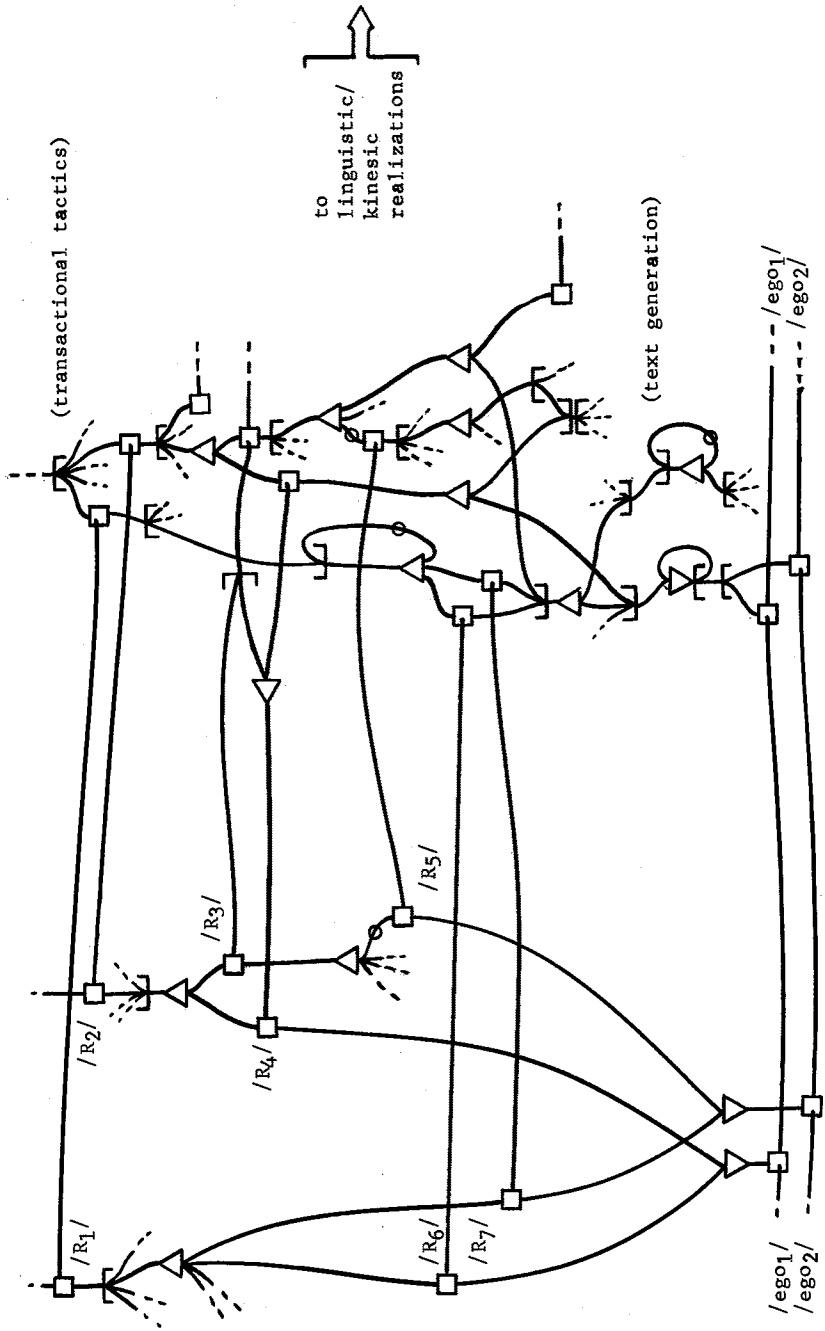

Figure 1: A sample social system with transactional tactics.

The form of these different strata have allowed us to make certain observations and predictions concerning human behavior and mental health. We believe that some mental health problems arise when individuals are involved in transactions which are inappropriate for the situation in which they are operating.[4] For example, in our earlier discussion of the secretary and her superior, the secretary is exhibiting behavior which is appropriate to roles originating in the Gemeinschaft-portion of American social structure while her employer expects behavior relating to roles originating in the Gesellschaft-portion.

Related to this situation is the problem of conflict; the individual is trying to exhibit role-behavior appropriate to different social systems at the same time. In simple terms, the transactional tactics is receiving more input than it can accommodate.[5]

In the past, we have used the example of Mary X in illustrating a form of conflict known as gender role confusion, in which an individual is either unaware of the role behavior of his or her sex, or aware but unable to exhibit such behavior.

Mary X has recently been hired by a large industrial firm as a systems analyst; she is a competent woman and does her job well. Unfortunately, some of the women with whom Mary works view her job as inappropriate to her sex role and label her as "aggressive" and "ambitious."[6] In our society, until recently, these labels were incontrovertable put-downs because they are associated with male role behavior. Having been "properly" socialized, Mary has this association encoded in her cognitive system. Thus when Mary overhears one of her co-workers say to another, "Oh, that Mary X is so aggressive!" we would not be surprised if Mary gets some psychological kick-back in the form of gender role confusion. A relational network diagram of this situation is given in Figure 2.[7] The cognitive input of this overheard sentence contains information which is not overtly indicated at the surface level, and it is this "implied" information which conflicts with Mary's identification of herself as female.

Where We're Going

Projections of our future work extend in many directions. We are intensely interested in applying cognitive analyses of sociolinguistic phenomena to psychotherapy/counseling relationships. The concept of role is crucial here; many hours of such relationships are wasted in attempting to free the participants from stereotyped role behavior and establish what the transactional psychologist terms an Adult-Adult relationship, one in which role-playing behavior is suspended in order to achieve a special type of communication.

The goal of some counseling relationships is to increase an

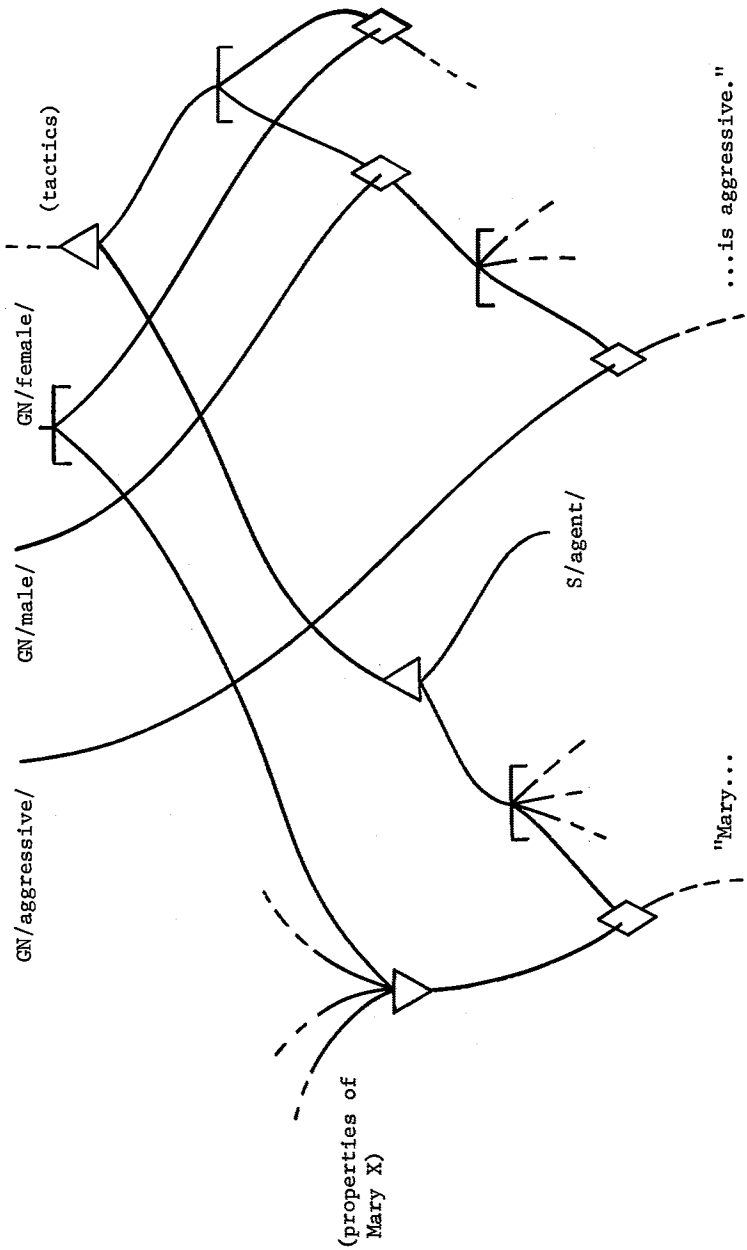

Figure 2: A tentative explanation of gender role confusion.

individual's awareness of himself and his inner feelings through the verbalization of such feelings. The linguistic expression of these feelings is often heavily disguised--either consciously or unconsciously; in this case, part of the counselor's difficulty in a particular relationship may be a matter of translating a client's symbolic language. Of course this is not enough; in order for the members of such a relationship to benefit from it, two-way communication must be established. In one sense, then, counseling may be characterized as a language acquisition process.

Counseling relationships often enable their members to re-define the relationships in which they are involved. Mary X may decide that aggressiveness has nothing to do with being male, just as being a systems analyst has nothing to do with being female; she will have rearranged a portion of her transactional tactics and its relation to her society's social systems to suit her own world-view based on her own needs and beliefs.

This ability to change the nature of transactions and their relations to other parts of the behavioral system highlights the status of the nodes involved in cognitive analyses as behavioral units (Reich 1973:91), meaningful only in relationship to the rest of the system of which they form a part.

The recent vogue for discovering "universals" in human behavior has perhaps overemphasized the place of such universals in the description of social and linguistic systems; this tendency must be carefully tempered so as not to introduce unnecessary biases and complications in description.

The English phoneme P/t/ cannot be equated with the Swedish P/t/, the Hungarian P/t/, or any other t-like phoneme in a different language. They all exhibit certain phonetic similarities, but their relationships to the rest of their respective linguistic systems are radically different. Similarly, an upper-middle class "family" in modern America and a "family" of Arunta aborigines may have nothing in common except the fact that someone somewhere decided to describe them with the same word! This caveat is not meant to discourage comparative studies but to increase their value by providing them with an appropriate framework.

We also feel that our work will be helpful in investigating not only language acquisition, but culture acquisition as well. Halliday in particular has concentrated on the complementary roles of society and language in a child's acquisition of his culture. It may be that the kinesic system develops at a different rate than the linguistic system; this would explain von Raffler Engel's findings that the kinesic realizations of many transactions appear before their linguistic adjuncts (von Raffler Engel 1975).

We believe that self-understanding is perhaps the highest form of consciousness an individual can attain, but we also believe that this achievement has no meaning unless it be coupled with an equally profound understanding of that which is outside the self. We hope that our work will contribute in however small a measure to both these goals.

Footnotes

* This paper was originally titled "A Linguistic Model for the Social Construction of Reality: Progress Report." We wish to thank Dr. David G. Lockwood, Phyllis Kneuppel, and Pat Whittier for their valuable comments, cirticisms, and suggestions. We would like to express special thanks to Dr. Victor H. Yngve and Dr. Robert J. Di Pietro for providing us with articles relating to our work.

1. "The Semotactics of Reality: A Sociological Discussion." The Second Foundation. Michigan State University, 1975. Available in mimeographed form. Correspondence concerning our work will be gladly received at either of the following addresses:

Michael Eric Bennett
Department of Linguistics
Michigan State University
East Lansing, Michigan 48824

Elizabeth Nall
Department of Sociology
Michigan State University
East Lansing, Michigan 48824

2. We wish to point out that the discussion here centers around American industrialized society, although this dichotomy is probably applicable in essence to societies with rigid caste systems as well. On the other hand, most "primitive" societies are most likely exclusively Gemeinschaft in character, although they may have Gesellschaft relationships with groups outside their own.

3. We are just beginning to work out the nature of kinesic realizations. It is probably that the kinesic system must be called upon not only to account for gesture and such per se, but also the location of transactions or parts of transactions in space, something which Pike has generally defined as field. Thus the kinesic realizations of some roles would be not only that two people shake hands when they meet, but also that they stand between three and five feet apart during the rest of the transaction.

4. Note that we do not intend to predefine the idea of "appropriateness" along the lines of what society regards as "normal". The concept of normality is too often put to bad use for us to bandy it around here. Mental health suffers when the individual is persecuted for engaging in activity which is regarded as "abnormal" or "unacceptable" by his society, although there may not be any substantial reasoning behind these opinions except the social forces of tradition and superstition. Also, "inappropriateness of transaction" may sometimes be a symptom of ill health rather than a cause of it.

5. We have ignored problems of timing in this paper, although we emphasize its importance in human behavior. It may be that

all transactions are organized around the .2 of a second beat which underlies human physiological systems (Di Pietro 1975: 53).

6. This is probably an instance of "contamination" of one social system by another. That is, analyzing systems has not traditionally been a transaction open to individuals playing female roles. Another instance of contamination is shown by the fact that the players of secretary and nurse roles are for the most part females.

7. This diagram is reproduced from our earlier paper, at which time we placed this aspect of psycho-sociolinguistic behavior at the sememic level. This does not affect the validity of our revised approaches, however.

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HOW BODY LANGUAGE IS TRANSLATED INTO WORDS

J. H. Prost and S. L. Smith

University of Illinois
at Chicago Circle

We asked several subjects to take poses which would portray various expressive, or emotional, states (e.g., happy, sad, angry, afraid, etc.). We photographed the poses. We estimated the angular positions of the major limb joints from the photographs (i.e., shoulder, elbow, wrist, hip, knee, and ankle). The measurements of the joint angles were etic descriptions of the poses. The expressive habits of the subjects were revealed from an analysis of the measurements -- the emic, or kinemic, categories of the subjects (Prost, 1974).

The photographs were shown to students from the Chicago metropolitan area. The students were asked to describe, in a single word, what they thought the poses were communicating. The verbal habits of the students were revealed from an analysis of their words, as word counts -- the emic, or sememic, categories of the students (Prost and Smith, 1975).

In the original work we did not cross-correlate the emic categories of the poses and responses because we were not sure how this should be done. Through discussions with our colleagues, during and after The First Lacus Forum, we discovered an appropriate method. In this paper we will describe this method and its results.

The poses in the photographs were etically described with angular measures of the positions of the large segments of the posers' arms and legs. The measurements, in degrees, specified the amounts of flexion-extension, abduction-adduction, and rotation manifest at each of the major limb joints, right and left sides. Twenty-two measurements, or variables, were taken on each pose. Any two poses can be compared, on the assumption that all the variables have equal weight, by computing the linear distance between them, using the square root of the sum of the squared differences rule. The more similar two poses are, the shorter will be the linear distance between them.

The linear distance computation is a method for discovering <u>nearest neighbor</u> poses. The nearest neighbor of a target pose is found by computing the linear distances between the target pose and all the other poses in the sample; the pose having the shortest linear distance to the target pose is its nearest neighbor. The next nearest neighbor, or second nearest neighbor, is that pose

which has the second shortest linear distance (designated as NN2, the nearest neighbor being NN1). The 3rd, 4th, 5th, etc., nearest neighbors can be found similarly (NN3, NN4, NN5, etc.).

Nearest neighbor comparisons are comparisons of overall similarity, ranked in terms of linear distance. Linear distance is a purely etic measure of comparison. This etic measure can be cross-correlated with emic categories. Emic categories group poses which have emic contrast or emic similarity.

If we ask someone to act "surprised" they might, hypothetically, flex their elbows 40^{0}. If we ask the same person to act "more surprised" they might flex their elbows 80^{0}. If we ask them to act "happy" they might flex their shoulders 20^{0}. The surprised and more surprised poses are kinemically similar -- both are intended to be "surprised" -- and the similarity is expressed, etically, by displacements of the elbow. Implied intensity -- "more" -- is, etically, translated into the amount of displacement. The two surprised poses and the happy pose are kinemically contrastive, etically expressed by the disjoint use of the elbow and shoulder. Kinemic contrasts contrast different joints. Our emic analysis of the habits of our real subjects demonstrates that this is how they structured their kinemic contrasts, similarities, and intensities, except that they used multiple rather than single joint combinations (Prost, 1974).

In the hypothetical example above the linear distance between surprised and more surprised is greater than the distance between surprised and happy, that is, the happy pose is NN1 to the surprised pose. What would happen if we had pictures of these three hypothetical poses and showed them to a respondent, asking him to guess the expressions of the poses? He might call all three poses "happy," misidentifying the surprised intentions of the poser, thus, missing the kinemic contrast between happy and surprised. The respondent might call the two surprised poses "playful" and the happy pose "startled," misidentifying the surprised and happy intentions of the poser but correctly identifying the kinemic contrast between happy and surprised. The respondent might call the surprised and happy poses "scared" and the more surprised "tense," missing both the kinemic contrast and the kinemic similarity. In the latter case we could say that the respondent used pure etic similarity for his sememic similarity since he gave the two cases with the smallest linear distance the same name.

With the real poses and responses of our subjects and students, we can assume the subjects and students were sincere in their intentions and 1) presume that when the subjects performed poses generated by the same command term (i.e., happy poses performed weeks apart) they intended a kinemic similarity and 2) that when the students used the same word they saw a sememic similarity (e.g., called two poses "happy"). In all other cases we can presume

they saw or intended sememic or kinemic contrasts. By using the joint angle data to compute nearest neighbor rankings we can compare the etic similarities of the poses to the emic similarities and contrasts of the subjects and students.

One of the subjects was a 13 year old girl, Kimmy, who posed for 535 slides (the Kc sample). The other subject was a 12 year old boy, Jack, who posed for 480 slides (the Jb sample). Each sample was shown to groups of students, 10 students per group, 80 slides per group. The students evaluated what the poses were communicating. If the posers were sincere their intentions were to communicate the expressive words given them by the photographer (i.e., afraid, surprised, sad, happy, attractive, relieved, pleased, tense, confident, sick, hate, bored, guilty, sexy, shy, thinking, tired, emotionless, relaxed, aggressive, pain, angry, embarassed, puzzled, and formal). As already reported (in The First Lacus Forum), the students guessed the intentions of the subjects 14% of the time for Kimmy and 16% of the time for Jack. To calculate these percentages we required that the students had to guess the exact word given by the photographer, that is, a happy pose had to be called "happy," or "happiness," but not "joyful," or "ecstatic," etc. This is a strict demand to place on the scoring of the students' responses.

We relax this strictness by ignoring the intentions of the posers and counting, instead, just the agreements between the students themselves. This is done by counting each occurrance per slide where a word is used more than once. This agreement is 43% for the Kc sample and 41% for the Jb sample. This is a measure of agreement for absolute kinemic similarity, or kinemic identity, because these agreements occur between students who had viewed exactly the same slide, and therefore, exactly the same pose. The students probably had, in their minds, some model of what happy, sad, angry, afraid, etc., should look like. The 43% and 41% measure the verbal equivalencies among these models. By the same measure, there is, roughly, 60% disagreement among these models.

Next, we look at the nearest neighbors and calculate agreements between them. Some of the nearest neighbors are poses generated by the same command word, for example, two happy poses performed on different days. The others are poses generated by different commands, for example, a happy and a sad pose. The poses generated by the same commands were intended to have kinemic similarity and the poses generated by different commands were intended to have kinemic contrast. The agreements for the NN1 for the Kc sample is 21% and for the Jb sample 19%. 35% of the Kc agreement and 29% of the Jb agreement is between nearest neighbors intended to have kinemic similarity; 65% and 71% is between nearest neighbors intended to have kinemic contrast. Thus, the percentage of agreement drops off as we go from kinemic identity to NN1 and a high percentage of agreement appears between cases meant to be kinemically contrastive.

The agreements for NN2, NN3, NN4, and NN5 on the Kc sample are 18%, 16%, 17%, and 14%, and for the Jb sample, 16%, 16%, 15%, and 15%, respectively. This shows that as poses become etically more different the percentage of word agreement decreases. The students are using pure etic similarity to decide on their sememic usage. The percentage of this agreement attributable to kinemic similarities is 31%, 40%, 32%, and 32% for the Kc sample, and 28%, 25%, 35%, and 26% for the Jb sample, respectively. This means that the ratio between agreements on kinemically similar and contrastive poses stays relatively constant throughout the NN1 to NN5 sequence. Since the ratio stays the same while the total agreement decreases, the students are using a function of pure etic similarity, linear distance, to decide whether two poses should be labelled with the same word. The students are not reading the kinemic similarities and contrasts. Why?

The answer is simple. The average distance, in degrees, for the 5th nearest neighbors is 77° for the Kc sample and 80° for the Jb sample. This is a 15° to 20° difference in each joint for the NN5 poses. People cannot identify anything less than a 5° difference and, in many cases, depending on perspective and the joints in question, have difficulty with differences of 10° (Prost, 1974). Subjects who consciously try to repeat a pose are lucky to achieve a 10° ($\pm$ 5°) replicability, joint by joint. So the first five nearest neighbors are 1) close to the level of just noticeable differences and 2) close to the human ability to replicate, or copy, postural configurations. At the levels of NN1 through NN5, the students were unable to distinguish similarities in joint combinations, the basis for kinemic similarities, and were obliged to use a general etic estimate of similarity for their verbal choices.

People have expectations about the appearance of the behaviors which exemplify the expressive words they have in their vocabularies and the closer a posture comes, etically, to one, or another, of these expectations the more likely it is to be so named. Because the postural field is continuous throughout (there are no discrete etic categories, or compartments) people must judge from experience how much etic difference they will allow before they assess two poses to be contrastive. The 60% disagreement at the level of kinemic identity represents, purely, disagreement among the students' expectations. The 79% to 81% disagreement at the NN1 level represents differences in the expectations as well as differences in the students' judgements as to the amount of difference constituting a contrast. The students must have decided, first, what is a happy, or sad, etc., posture and, second, how much variability they might expect in its performance.

When people want to be "more" expressive they keep their combination of joint displacements consistent but vary the amounts of displacement. We gave a hypothetical example of this using <u>surprised</u> and <u>more surprised</u>. With Kimmy and Jack, from time to

time during the photographic sessions, we gave them the command to act more emotional, that is, "act more happy," "act more surprised," "more sad," etc. These "more" commands always followed directly their equivalent normal commands, so that the subjects would have the opportunity to make their kinemic similarities clean and distinct. The average linear distance between the normal and the more poses for the Kc subject is 136° and for the Jb subject 115°. These etic differences are greater than the 77° and 80° found for the same subjects at the NN5 level. With the more poses, we had experimentally forced the etic difference to increase simultaneously retaining a kinemic similarity.

The word agreement between the more poses and their companion normal poses is 34% for the Kc sample and 33% for the Jb sample. This should be compared with the 43% and 41% for kinemic identity and the 21% and 19% for the NN1's. The percentages prove that as performers pass the point of just noticeable difference and gain enough etic distance to clearly differentiate their kinemic contrasts, they can, and do, control word choices with their body language.

We allowed students to see only 80 slides, maximum, and, therefore, no student could learn the kinemic contrasts and similarities of the subjects. If a single student responded to the entire sample his verbal decisions might correlate more strongly with the subjects' kinemic categories because he could learn from each successive slide how the subjects were composing their kinemic combinations. We have not yet tested this hypothesis.

When we prepared the sample for the students' viewing we separated the more cases so that no more pose was in the same set of 80 as its partner normal pose. The 33-34% agreement for the more poses is, therefore, proof that the nature of the kinemic similarities, that is, joint combinations, and the embedded intensity scale, that is, amount of joint displacement, was an a priori expectation of, and criteria for, the students' responses.

Peoples' expressive words can be defined in terms of an etic, purely behavioral, definition (i.e., a set of joint angles) and a statement specifying the variability which circumscribes verbal identities, adjusted for a scale of expressive intensity. This defines the expectations of a speech community, recognizing that members with experience can modify their criteria to fit the habits of particular performers.

Computing services used in this research were provided by the Computer Center of the University of Illinois at Chicago Circle. Their assistance is gratefully acknowledged.

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V

HOW LANGUAGE IS USED

A SURVEY OF RESEARCH ON LANGUAGE DIVERSITY: A PARTIAL WHO'S WHO IN SOCIOLINGUISTICS

R. Paul Murphy
Arizona State University

Jacob Ornstein
The University of Texas, El Paso

This article is a revision and updating of a previous survey of the same (sub)field done about a year ago by the same authors. When asked for their help, several of our colleagues suggested improvements for the new version. If the result is clearer in its direction and more accurate in its presentation, a good deal of the credit goes to them.[1]

I. INTRODUCTORY REMARKS

In the 14th Century, William of Ockham proposed his law of parsimony. He advised scientists to continue to assume that things were simple until they were driven to assume otherwise. Recently the effects of "Ockham's razor" and the boundaries of disciplines relating to language has been much debated. In an older tradition, Leonard Bloomfield (1933) ruled that matters of "meaning" best left to philosophers, psychologists, anthropologists or other scholars should not be considered a part of scientific linguistics. Thus he purposely limited the scope of his inquiry and stressed objective analytical procedures in order to establish linguistics firmly and separately among the behavioral sciences. A little over twenty years later, Noam Chomsky (1957) saw no virtue in Bloomfield's mechanical procedures and no sense in much of his objectivity. For the transformationalist, linguistics must be broad enough to account for many of our (or his) intuitions about language. The Bloomfieldians took Ockham's razor seriously, but their results were never really simple. Chomsky's followers have made linguistics decidedly less simple. The trend continues, and now many of Chomsky's followers accuse him of arbitrariness and superficiality. One group holds that, even though the more recent theories have dealt more fully with the issue of meaning than did the structuralists, the semantic component will require still more power to reflect "real world" conditions. Other scholars are returning to a more empirical orientation, but one focusing on the integral role language plays

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[1]The previous article, "Models and Approaches in Sociolinguistic Research on Language Diversity," appeared in the April 1974 issue of Anthropological Linguistics. Where we may have failed to make the best use of our correspondents' advice, we assume the full responsibility. Our thanks to: Basil Bernstein, Garland Bills, Andrew Cohen, Susan Ervin-Tripp, Peter Farb, John Gumperz, Einar Haugen, William Mackey, Roger Shuy and Frederick Williams.

in so many aspects of our lives. It is the latter, more behavioral, emphasis which will concern us here.

We shall not have much to say about the semanticists, but the new empiricists present multiple problems of classification. A few years ago, psycholinguistics came into fashion. Interest has not died out in either language acquisition, the effects of language on perception and cognition, or any other areas of psycholinguistics, but it seems that sociolinguistics is the glamour area at present. Actually, there is no clear dividing line between the two new disciplines. One way or the other, both must deal with individuals as well as groups and both must recognize neither emphasis as truly divorceable from the other.

What is left is a modern Ockham's razor with slick multiple settings and new linguists willing to experiment with the new instrument. Linguistics is in an exploratory phase. Now it is acceptable to be somewhat arbitrary and preferable to admit it. For our purposes in this review, we admit that our survey does not take in all of sociolinguistics and that what it does take in is not all purely and exclusively sociolinguistic. We do try to survey research that stresses group norms over individual performance and features communities of some complexity. Within those restrictions, we admit a great deal of variety.

II. ORGANIZATIONAL CONSIDERATIONS

Our last survey divided research into three broad groups. Walt Wolfram (Federal City College) originally suggested the categories personally to Jacob Ornstein. In a recent text done with Ralph W. Fasold, Wolfram reaffirms the ongoing presence of these emphases in today's sociolinguistics. In their Preface, Wolfram and Fasold (1974) divide the field into four trends. The first two, the _sociology of language_ and the _ethnography of speaking_, are still so clearly related that our subsuming them under the first heading would be as valid now as then. Our 'linguistically-oriented sociolinguistics' corresponds to their _general linguistics_, and 'intermediate studies' is roughly the counterpart of the _ecology of language_.

Einar Haugen, Professor Emeritus (Harvard) and the researcher who coined the term 'ecology of language,' has been firmly critical of our intermediate category. In a personal letter, he argues we should never have used the section as a catch-all or mistakenly referred to work within it as a "compromise." The fact is, he says, the orientation need be neither intermediate nor a compromise; it is simply pragmatic and "data-oriented." We agree that to the extent that our previous first two categories stressed discrete, not obviously reconcilable, theoretical positions, reference to the third even as "intermediate" made its practitioners appear wishy-washy and their work seem without purpose. Not desiring an empiricist vs. rationalist debate, we recognize that scholars of the school that emphasizes data were going their own way and amassing a great deal of valuable information long ago. They generally have tried to see socially

and linguistically complex situations as clearly as possible from a common-sense perspective, one which has allowed for intuitive insights but by no means been sloppy. In more cases than not, these pragmatic linguists have also been "applied linguists," vitally interested in the large issues of language planning or education as well as the purer pursuit of knowledge. We regret our casual mention of compromise.

We did little justice in our other paper to the forerunners of present-day sociolinguistics. Accordingly, we are adding a brief discussion of select research published in the United States more than fifteen years ago clearly addressing itself to concerns that now are central to sociolinguistics. While it cannot be comprehensive, it can still be informative.

III. SOME PRECURSORS OF SOCIOLINGUISTICS

American linguistics have not just recently become aware of the speech variation of complex societies. One of our earliest linguists, W. Dwight Whitney, cautioned his colleagues (1881) to study variation carefully. He warned them not to become so preoccupied with such theoretical arguments as the existence or nonexistence of "mixed" languages that they might "look only on /their/own side of the shield." In Language, Bloomfield had advanced a typology of linguistic borrowings. His position on theory was characteristically straightforward: what he could not fit easily into it (eg., Pidgins and Creoles) he merely dismissed as "aberrant." The anthropological linguists of the 1940's and 50's published a good deal on the application of the then-current concepts of -emes and allo-'s to American Indian languages. It is easy to see, though, that other aspects of their familiarity with these languages and people interested them--aspects which could not be fitted gracefully into the structural tradition--and they developed a whole literature on 'linguistic acculturation' (see Johnson 1943; Trager 1944; Casagrande 1954-5). Ample precedent can be found for the data-orientation favored by Haugen.

A separate, vital literature--drawing upon the theories of modern linguistics, but not limited to them--has long centered on the social and psychological aspects of widespread bilingualism. EINAR HAUGEN is a giant in that literature. His major work, The Norwegian Language in America, may have been published in 1953, but his interest in bilingualism considerably antedates that. In 1948 he had offered the first course on bilingualism ever available at a summer Institute of the Linguistic Society of America. Furthermore, many of the findings included in the book on Norwegian were drawn from studies begun as long before as 1936. A landmark paper in 1950 expanded the existing typology of borrowings. His generalizations have always been supported by examples, and he never has lost sight of the important human factors that have lead to the changes in phonology, syntax or lexicon in a minority language. More recently, another major publication has borne witness to Haugen's (1973) ongoing interest

in studies of a wide range of speech communities and languages.

The appearance of Haugen's work on Norwegian would have been enough to make 1953 a banner year in the study of bilingualism, but that year also produced the important Languages in Contact, by URIEL WEINREICH. Just the bibliography and list of languages sampled would suffice to attest to Weinreich's independent grasp on his field; still he frequently acknowledges his indebtedness to Haugen. Since the 1950's, 'languages in contact' has become practically a synonymous term for research into the language customs of bilingual communities.

While Weinreich paid close attention to data, his was not the data-orientation of Haugen. Whereas Haugen concerned himself with an in-depth treatment of the speech behavior of a specific community, Weinreich sampled more languages, perforce becoming more theoretical. These incipient theories have been incorporated into much of recent psycho- and sociolinguistics. The applied linguists have turned frequently to Weinreich for support--indeed, contrastive linguistics owes him a great debt.

Most introductory linguistics texts include a section on the early 20th-century development of dialectology in Germany and France. By the late 1930's, a parallel interest in the study of regional speech in this country began to result in the piecemeal publication of the Linguistic Atlas of New England, a large cooperative venture directed by Hans Kurath. Similar works have reached various stages of completion and publication presenting details of the speech of the Upper Midwest and Gulf States. In all the Atlas work, the principles of study developed earlier in Europe have supplied the model, and regional distribution of pronunciation and lexical variants has taken precedence over any thorough investigation into syntactical and stylistic variation. In addition to Kurath, the names of Harold B. Allen, Bagby Atwood and Raven I. McDavid are often associated with early efforts in American English dialectology. (See Allen and Underwood 1971 for essays tracing this tradition from its beginnings through to its more recent shift of emphasis from regional to social factors in accounting for variation.)

IV. THE STUDY OF LANGUAGE DIVERSITY IN THE SOCIAL SCIENCES

A. ANTHROPOLOGICAL LINGUISTICS

It is pushing the boundaries of sociolinguistics back a bit to include within them a 1959 paper by CHARLES A. FERGUSON, the former Director of the Center for Applied Linguistics, now at Stanford University. For two reasons, the liberty makes sense: 1) the title of the paper has developed into an essential term with many related assumptions in sociolinguistics, and 2) the author has collaborated with the other anthropological linguists to be introduced soon in this section. Building on the notions of High (H) and Low (L) verities (cf. Bloomfield 1933), Ferguson introduces the term 'diglossia' to characterize the language conventions of communities in which "two or more

varieties of the same language and used by some speakers under different conditions" (325). The languages providing his data include Modern Greek, Swiss German, Arabic, and Haitian Creole. In each case, an H variety is in widespread and relatively stable contact with a complementary L. Seemingly universal properties emerge for the varieties: the communicative function at hand often determines the correct variety; the prestige of the H is so much greater than that of the L that informants often claim there is no L at all, that H is more beautiful, logical and appropriate for refined purposes than any vulgar style of speech would be; the H figures more prominently into the literary tradition (if the L is written at all); L is acquired at home and H taught at school; H is more highly standardized; diglossia tends to be maintained with a high degree of stability, with borrowing primarily going from H to L; the grammar of H usually includes categories not found in L; the lexicons of the two varieties include many pairs with the H or L items immediately recognizable, as well as some specialized items in each variety relating more to the social domains appropriate to it; and generally the phonology of the L is the basic one with certain divergent features forming a subsystem for the L. It is evident that some of these observations are more linguistic than sociological and others vice versa. The important contribution of the term 'diglossia' lies in the recognition that there are, in all bilingual societies, particularly defined situations in which either of the codes available is more likely to occur.

Convinced that despite the efforts of anthropological pioneers such as Boas, Malinowsky and Sapir, there has remained a serious gap between what is usually described in grammars and what is found in ethnographies, DELL HYMES has conceived of a new cultural study which he calls the 'ethnography of speaking.' The term was first used as the title of a programmatic essay (1962) wherein he attacked the prevailing relativistic orientation of cultural anthropology. Too often anthropologists had avoided worthwhile investigations of the functions of linguistic variation (either between or within languages) in their fieldwork. Even where his colleagues had treated some aspects of functional shifts, Hymes believed they had usually supplied in their data the appearance of a greater homogeneity than is likely to be found anywhere.

Hymes has consistently argued that if linguists would only broaden their concept of what is systematic in language, there would be no need for any new names for "new" disciplines--"linguistics" would do. In 1967 he provided a more explicit blueprint for the elements essential even to any preliminary ethnography of speaking. 'Speech situation,' 'speech event,' and 'speech act,' terms introduced in an embryonic form in the previous article, were more fully explained and illustrated. Hymes proposed that it should be as vital a concern of the humanistic linguist to determine the structure and social-psychological reality of these terms as it had been to establish precision in the search for

things -emic in the structural tradition. "In short, there must be a study of speaking that seeks to determine the native system and theory of speaking" (13). In an ingenious mnemonic device, SPEAKING, he suggested a few primary components essential to any speech event, and presumably essential to some future model of generative grammar.

From his position at the University of Pennsylvania, Hymes has influenced sociolinguistics since its beginnings. His writings and lecturing have brought him to the international attention of scholars, and his journal, Language in Society, has provided sociolinguists with a highly respected forum.

Another important social anthropologist, and a frequent coauthor-coeditor with Hymes, is JOHN J. GUMPERZ (the University of California at Berkeley). At about the same time when Hymes was calling for the more complete study of function in language, this investigator was well into highly functional accounts of language patterns in an impressive range of multilingual communities. He conceived of the 'linguistic community' as a political entity united by an ideal standard of language along with an interconnected system of subcodes, all with their appropriate functions and all forming a 'communication matrix' (1962). Believing that subgroups in any community (whether monolingual, bilingual or multilingual) tend to develop their own strategies for communication and norms for usage, Gumperz has emphasized the study of small groups as the best way to get at those norms and the process which develops them. Whether working with the effects of caste in India or education on mainland Puerto Ricans, he has maintained that select demographic factors are of central importance in establishing and maintaining the characteristic linguistic production of a community's subgroups.

Speaking at a Georgetown University Round Table, Gumperz (1965) proposed the 'linguistic repertoire' as the sum total of recognizable varieties--styles, codes, styles within codes, whatever--which have a demonstrable set of functions within a given community. In line with his search for varieties, he has refined our perceptions of 'code-switching' and the 'verbal strategies' which motivate it. One notes a deliberate generality in Gumperz terms. He would like to develop a framework wherein the universal properties of communication in communities of all degrees of social and linguistic complexity might easily and meaningfully be compared. In his field methods he blends the objective judiciously with the subjective. Rather than simply to ask informants to talk about variation off the top of their heads, he gets them to respond to taped samples of actual speech from their communities--speech which the analyst has reason to believe contains code or style shifts. It then is up to the informants to recognize whatever varieties they believe are present as well as to generalize upon whatever uses they can perceive the varieties being put to.

B. SOCIAL PSYCHOLOGY

JOSHUA A. FISHMAN (Yeshiva University) has been to social

psychology what Hymes and Gumperz have been to anthropology--perhaps not the first fieldworker ever to see the potential of language study to the theory of his own discipline, but surely a pioneer in the development of modern sociolinguistic theories. Bilingualism in the Barrio is the sprawling final report of government-sponsored research done as a team effort under Fishman's direction. The sample includes 431 Spanish-English speaking subjects representing 90 families drawn from three adjacent blocks in Jersey City. The subjects were mostly young unskilled workers with little formal education and of a relatively low socioeconomic status. Contrasting stratified samples were also taken of intellectuals, high school students, and other groups from the extended Puerto Rican community ranging into New York City. Extensive ethnographic information was gathered on all subjects, including demographic data and details of their life styles.

The elicitation battery was extremely broad, including self-report on language fluency, language loyalty and the like. Some open-ended interviews supplemented this data, particularly to establish the relative usage of English vs. Spanish in each of Fishman's five hypothesized 'domains' of home, neighborhood, church, school and work. The attention of the researchers was constantly directed to a small inventory of linguistic details from both Spanish and English. The performance of each subject was rated by independent judges according to: accent, reading skill, writing skill, Spanish repertoire range, and English repertoire range. The informants were further asked to make value judgments about the personal characteristics they suspected were appropriate to speakers they heard on taped samples. (Refer to the following discussion of Wallace Lambert's 'matched-guise' technique.) Particular strengths of the barrio study include the relative importance it affords to domain and its interaction with the multiple varieties coexisting in a bilingual community. Elaborate statistical techniques permitted the simultaneous evaluation of numerous socioeducational and linguistic variables.

Another social psychologist, WALLACE E. LAMBERT (McGill University), has long sought to define the place of language in the development and perpetuation of socially-held attitudes within a multi-lingual community. Believing the process of social stereotyping to have a strong linguistic component, he has devised a method (described in several essays in Lambert and Gardner 1972) for revealing the nature and strengths of prejudices triggered in subjects as a reaction to the speech of others. Subjects participate in a discussion of how we all have been known to form impressions of someone else based, for example, on nothing but the voice cues we have gotten from a telephone conversation. The subjects are then asked to record their impressions of persons they hear speaking on tapes. Sometimes what the "judges" hear is in one of the community's codes and sometimes it is in another. They scale their reactions on a continuum spanning two polar adjectives (e. g. interesting vs. boring,

successful vs. unsuccessful, etc.) in a manner drawn from the Osgood Semantic Differential technique. What the subjects are not told, and seemingly few guess, is that certain of the voices they hear are doing double duty. Scattered randomly in with the other voices will be these 'matched guises,' and the data from these provide the insights into the between-group stereotypes current in the community. The variables of age, sex, amount of education, and a number of others--when applied both to the judges and to the voices they judge--have turned out remarkably reliable results in test after test. In addition to their initial studies of French-English bilinguals in Canada, Lambert and his associates have worked successfully with bilingual and dialect speakers in the United States.

In another set of experiments, Lambert has demonstrated that social factors strongly influence the strengths and qualities of motivation one group is likely to have to learn the ways of speech used by another. (Again, see Lambert and Gardner 1972.) As the resultant learning is likely to vary predictably with the nature of the learner's motivations for learning, these findings have enormous implications for both bilingual and bidialectal educational programs.

Yet another social psychologist, one as often known as a psycholinguist as a sociolinguist, is SUSAN ERVIN-TRIPP (Berkeley). In one experiment (1964), she investigated a group of Japanese war brides married to Americans in comparison with socioeconomically matched groups of monolingual Anglos and Japanese women tested in the U.S. and Japan. Detailed ethnographic descriptions were made for all subjects. A Japanese-speaking worker then interviewed each subject twice, in the same setting, first in Japanese, then in English, getting the responses on tape. The elicitation battery included, in addition to open-ended interviews, semantic differentials, narration or description of given events, and thematic apperception tests.

Of the independent variables, language was judged to be the most influential. Clearly the bilinguals evidenced a tendency to respond more like their monolingual counterparts when functioning in either of their languages. When speaking Japanese, for example, both the Issei (Japanese born) and the Nisei (American born) women gave more typically Japanese responses, whereas when speaking in English, the bilinguals replied in a manner more typical of the monolingual American women. The evaluator must keep in mind, however, that the social dimensions were not to be overlooked; the idealized picture was complicated somewhat by the introduction of topical variables and the change of interlocutors. It was difficult for bilinguals to discuss certain topics in what they considered to be the inappropriate language. Furthermore, many of the subjects found it nearly impossible to avoid Japanese completely (regardless of topic) when speaking with a fellow speaker of Japanese.

The bulk of Ervin-Tripp's 1964 article--the same one in which she describes the Japanese experiment--provides an excel-

lent companion piece for Hymes (1967) in that many of the socially-relevant variables which Hymes later worked into his SPEAKING "model" appear here in a slightly different arrangement, with different examples.

C. SPEECH

FREDERICK WILLIAMS is the former Director of the Center for Communication Research at the University of Texas (Austin), and present Dean of the Annenberg School of Communications, U.S.C. He shares Lambert's interest in stereotypical community-wide attitudes toward languages and their speakers, but has added his own refinements to the tools taken from Lambert. In an experiment completed with Naremore (Williams 1970b), he borrowed samples of speech from Roger Shuy's Detroit Dialect Study (to be mentioned later). Forty children were included; twenty of them from a lower socioeconomic background and twenty from the middle class. A group of judges, some Black, some White, and all teachers, were asked to rate the speakers on 22 separate dimensions, matching them with what they believed to be the highest possible vocations the speakers could hope to attain. In line with the general findings of Labov's research (to be described later), at times a single linguistic feature would suffice to predict the responses of the Black or the White judges. Williams has come to criticize much of the ethnocentrism and prescriptive attitudes implicit in the curricula of many American schools.

In his more recent work, Williams (1974) has expressed some misgivings with Lambert's methods. He has speculated that the concept of attitude cannot really be captured on the single dimension of the average scores Lambert derives from semantic differential testing. Using a modified statistic procedure, Williams now plots his result bidimensionally on a graph representative of what he terms a subject's 'latitude of attitude.' The advantage of this procedure is that it is capable of capturing the dynamics of changes in stereotyping as they occur. Newbury House will soon publish a compilation of Williams' attitude studies.

D. SOCIOLOGY

The theories of BASIL BERNSTEIN, a sociologist at the University of London, has been a sociolinguistic force to be reckoned with--if not always agreed with--since the early 1960's. Characteristic of many but this author's earliest works has been a tendency to argue abstractly, without always including data to support his theoretical claims. The bulk of his extensive publications relate to his observations of working and middle class British schoolchildren. He has consistently proposed that social stratification is accompanied by correspondingly distinct orientations toward the use of language.

In the case of working class children, Bernstein (1972) has observed relative limitations and "predictability" in both their word choice and syntax. The middle class children, on the other

hand, have impressed him with a greater concern for the relationships of things around them and their tendency to develop stronger, more precise verbal means to make their perceptions clear to others. His terms 'restricted code' and 'elaborated code' apply to the working and middle class orientations respectively, and have attracted linguists, social scientists and educators alike.

Bernstein's work undeniably has a strong theoretical component qualifying it for inclusion in this first, social-theoretical division of our essay. It offers many suggestions for education (especially where it is accompanied by more than the usual meagre date), and could easily qualify as well for inclusion in the last, more pragmatic, section. Does Bernstein imply, for example, that the chances for linguistic and vocational success of society's less favored (?) groups should discourage educators? That question will return later.

E. LINGUISTICS

Another linguist-sociologist-educator whose work is as practical as theoretical is WILLIAM LABOV, now of the University of Pennsylvania. More than anyone else, Labov belongs in all of our categories--no one has contributed more to all of sociolinguistics than he. He was well into social psychology while still a graduate student in linguistics at Columbia University. His study of the language patterns on Martha's Vineyard (1963) of course antedated, but more significantly formulated his methodology for, his later work on the social stratification of English in New York City (1966). The clever application of statistics and cleverer insights into which of all possible social factors to correlate with chosen linguistic variables allowed him to demonstrate (both with the New Englanders and the New Yorkers) that one's speech is closely tied to one's aspirations and feelings about his community. The theories developed in the first study and perfected in the second have profoundly influenced most such studies since. Labov the contributor to emerging variation theory and Labov the humanistic educator will appear again and again throughout our survey.

Among the most valuable publishing projects thus far in the 70's is the Language Science and National Development Series edited by Anwar S. Dil (U.S. International University) and published by the Stanford University Press. The format is consistent: Dil chooses a scholar of real and proven importance to sociolinguistics, prepares a short, informative introduction to the volume--including details on the education and most significant achievements of the author. Next, in chronological order, are the principal journal articles (in good clear print) and at the end of the volume, the honored author generally responds with a few pleasant words about the series and a broad indication of where his or her present work is leading. The calibre of contributors is very high. Membership includes: Ervin-Tripp (1973), Ferguson (1971), Fishman (1972b), Gumperz (1971), Haugen (1972), and Lambert (1972).

As we have already admitted, sociolinguistics is not limited to studies on diversity in language. Joshua Fishman (1972) has borrowed a useful distinction from sociology separating the "micro" concerns of individual and small group interaction from the "macro" concerns of large group norms. In discussing the importance of constructs such as 'domain' for both 'micro-' and 'macrosociolinguistics,' he concludes (453): "It is one of the hallmarks of scientific social inquiry that methods are selected as a result of problem specifications rather than independently of them." This is Ockham's razor again. Even though we agree with Fishman that many arbitrary differences will eventually disappear as new constructs are discovered to unify branches of sociolinguistics, there still seems to be some utility in limiting our review to inquiries into language diversity. In so doing we exclude some important research. The work of Brown and Ford (1961) into the forms of address used in American English, John Fischer's study (1958) of social influences on "the dropping of final g's" from English verb forms, and the investigations of Sacks, Schegloff and Jefferson (1974) into turn-taking in normal conversation are but three examples of research which does not exactly emphasize language diversity but which surely would have a place in any integrated theory of the sociology of language.

V. LANGUAGE DIVERSITY AND LINGUISTIC THEORY

"Scientific" has been a favorite adjective of both the structuralists and the transformationalists, but the fundamental differences in their conceptions of science are well documented in Chomsky 1957. While they have differed, both traditions have tried to replace informal with formal statements about language. In keeping with a scientific preference for things formalizable, it should not be considered unfairly prejudicial on our part if we rate the progress of linguists in building sociolinguistic theories more highly than we rate that of the sociologists of language. The generative theories of the past two decades already have developed much of the sort of integration of elements and explanatory power which could conceivably allow for the incorporation of more of the observed effects of social conditioning upon language within a formal structure than could anything that comes to mind in the social sciences. The concerns of humanists naturally lead to more of those difficult initial grapplings which may well be necessary but which, for the time being, lack something of the feeling of direction that can be found in linguistic-based sociolinguistic theory. Whereas the theorizing of the scholars in the last section was sometimes a kind of mutually supportive brainstorming, the theorizing of the scholars surveyed in this section tends to be competitive, rigorous and even combative.

One of the most encouraging developments in variation theory, as it exists within linguistics, is the founding of its own new forum. Georgetown has been the first American university to offer a degree program in sociolinguistics. Along with

that commitment has come another: an annual conference on "New Ways of Analyzing Variation in English--NWAVE" with the proceedings published by Georgetown's own press. These NWAVE conferences now number three. Bailey and Shuy's dedication of the first set of NWAVE papers (1973) will lead us nicely into our discussion of variation theory: "To William Labov, who freed us from static analysis."

A. WILLIAM LABOV AND VARIABLE RULES

Labov is something like the Chomsky of variation theory, very much respected even by those who have grown to disagree with his original theories. While at Columbia, he took on Uriel Weinreich's dissatisfaction with the prevailing synchronic emphasis in linguistics. These feelings led him to the Martha's Vineyard study reported in 1963. Statistical measures enabled him to perceive some linguistic changes <u>in progress</u> rather than in retrospect; but how were these sociological observations to be worked into linguistic theory?

There was the start of the answer in Labov's concept of the 'linguistic variable.' This construct grew out of his conviction that Chomsky had been wrong in assuming that the linguists's own intuitions could lead him to the meaningful fabrication of a model of the competence of an 'ideal speaker-hearer' --a linguistic unicorn operating in a completely homogenous speech community. Labov has since (1971:162) stated flatly: "the search for homogeneity in intuitive judgments is a failure." While all might not be flux in usage, too much is flux to get far being introspective about it. Linguistic variables are select, high-frequency variants--usually, but not necessarily phonological--which turn up so frequently and unobtrusively in all levels of everyone's speech that they can neither effectively be avoided nor purposely distorted. When these variables are correlated with such demographic factors as age, sex and socioeconomic status, and then further correlated with factors relating to style and discourse, meaningful patterns of usage emerge for the speech community.

The 'variable rule' first appeared in the exhaustive discussion of the findings of Labov, et al. 1968. Such rules were hypothesized to supplement the bulk of other rules, mostly invariant, in a grammar and thereby specify the effects of the total environment--linguistic and sociological--on the output of the grammar. The research indicated that New Yorkers realize /dh/ and /th/ under certain conditions not as continuants but as the highly stigmatized stops /d/ and /t/, respectively. Labov formulated the appropriate variable rule this way:

$$\begin{bmatrix} +\text{ cons} \\ -\text{ voc} \end{bmatrix} \longrightarrow \left(\left[-\text{ cont}\right]\right) \Big/ \begin{bmatrix} -\text{ strid} \\ -\text{ back} \\ +\text{ cor} \end{bmatrix}$$

This rule departs from the standard Chomsky-Halle format only by

the additional convention of the parentheses around the feature to the right of the arrow, the parentheses indicating variability. For each such rule, the grammar must be supplied with an index of variability, $0 = 1 - k_0$. Assuming that a rule actually were 'categorical,' that is, to be applied universally with no effective constraints, the value of k_0 (the contextual constraints) would be zero. However, this would not be the case for variable rules, and the degree of variability would be expressed as a function of the constraints of both style and socioeconomic class membership, as in the following formula: $k_0 = f(\text{SEC, Style}) = a(\text{SEC}) + b(\text{Style}) + c$. The precise values inserted into the formula for each linguistic variable in each rule would result from the meticulous observation of actual speakers drawn from the speech community and functioning in real situations.

B. DAVID DECAMP AND IMPLICATIONAL SCALING

Since the late 1950's, DAVID DECAMP, recently appointed Director of the International Program at the Center for Applied Linguistics, has been a prominent member of a school of linguists interested in the analysis of pidgin and creole languages. Earlier, we cited Bloomfield's 1933 reference to these varieties as "aberrant" mixtures. The varieties were known to exist, but DeCamp (1971a) finds that only recently have they been taken seriously as data for linguistics.

DeCamp (1971a:15) characterizes a 'pidgin' as "a contact vernacular . . . normally not the native language of any of its speakers /and/ used in trading or in any situation requiring communication between persons who do not speak each other's native language." Pidgins in many parts of the world, based on many different languages, share characteristic reductions of grammatical categories found in their source languages. As they gradually acquire native speakers, pidgins gain what Stewart (1968) terms the feature of 'vitality,' thereby becoming 'creoles.'

The foregoing quasi-definitions and sketchy characterizations of these remarkable language varieties are so oversimplified as to be useless except to illustrate the social and linguistic components vital to any understanding of them. The interest of creolists in language variation is natural, their concern for developing a theory of variation nearly inevitable.

After a half a decade of debate, 'implicational analysis,' DeCamp's formalization of variation theory, remains the only true alternative to Labov's variable rule format. This creolist has all along acknowledged a strong but coincidental similarity between his type of linguistic analysis and the 'scalogram' statistical procedure developed by Louis Guttman for use in sociology. According to DeCamp (1969) the chief advantage of implicational linguistic theory, as opposed to variable linguistic theory lies in its relative ease of accomodation to the preexisting transformational-generative format.

The familiar problem of how big a job to take on with our speculations--Ockham's razor--seems to separate those in Labov's camp from most of the creolists. Perhaps because he is a creolist, DeCamp has seized upon one theoretical consideration as leading most directly to his dissatisfaction with Labov's treatment of variation. He says that just as the transformationalist feels compelled to add the potential of recursion to his grammars, based partially on the belief that any child commands just such a creative aspect of language, he also should feel obliged to account for any child's ability to recognize, evaluate and sometimes produce an indefinite number of 'inter-idiolectal code switches.' Stated more simply, a child can understand the meaning and social significance of a large number of specialized styles and registers--even some of which he would never himself have occasion to use. This too is a creative principle and needs attention in an adequate theory of competence.

According to DeCamp, Labov's method of handling variation is essentially performance-oriented. Rather than concentrating on the specification of an abstract community-wide competence, it indexes actual idiosyncratic performance data. Rather than being deductive, as transformationalists generally would claim to be, it is inductive. Furthermore it is circular in that it uses sociological data to explain the significance of essentially the same sociological data. It offers description rather than explanation.

For DeCamp, it seems wrong to suggest that linguistic features vary independently. He argues that certain aspects of the code actually vary as a package to convey whatever effect the speaker has in mind. The situation is analogous to the conventional scheme of feature analysis with its tightly controlled redundancy rules. When dealing with nouns, we know that a feature [+abstract] also implies the features [-animate, -human, -masculine] as well. The features are more easily managed and learned because they fall into a hierarchical array. By the same token, DeCamp believes all the stylistic varieties in use within any speech community might be rank-ordered along the same sort of continuum that Shuy and Wolfram have proposed for dialects. Then stylistic features, such as [±pompous] could affect the appropriate lexical, phonological and syntactic variations necessary throughout the grammar to result in, as well as explain, just the sort of variation which Labov has thus far only described. A choice of [+pompous], for example, would also imply a choice of [+formal, -casual]. Higher level features in the deep structure would cut to the proper stylistic level to call forth the appropriate variants in their appropriate proportion to all others.

The following table depicts DeCamp's (1969) system for relating features to varieties:

Features

F_1	F_2	F_3	F_4	F_5		
1	1	1	1	1	V_1	
1	1	1	1	0	V_2	
1	1	1	0	0	V_3	Varieties
1	1	0	0	0	V_4	
1	0	0	0	0	V_5	
0	0	0	0	0	V_6	

If the value of any square (the intersection of features and styles) is 1, any square above or to its left will also be 1; if 0, any square below or to the right will also be 0. Thus one can predict that if a speaker uses the variant pronunciation vahz for 'vase' he will almost certainly say It is I as opposed to It is me as well.

At least one reasonably large scale investigation of actual language data drawn from a viable speech community has actually been undertaken within DeCamp's basic theoretical framework. Stolz and Bills (1969) achieved considerable success differentiating CTRWASPNSE (Central Texas Rural, White Anglo-Saxon Protestant Nonstandard English) from the local SCTE (Standard Central Texan English).

C. VARIABLE RULES WITH IMPLICATIONAL SCALING

After briefly reviewing the principles of the two conflicting models, RALPH W. FASOLD (1970) observes:

> It would be legitimate to ask each group of researchers how they account for the insights revealed by the other approach. Implicational analysis expressly avoids tabulations of variability, while research which deals with variability does not directly show implicational relationships.

He wonders how the researcher might derive the best of both worlds.

Stolz and Bills, he notes, were forced to establish various arbitrary 'thresholds' for many of their features. Binarity is basic to implicational analysis, and yet there were situations where it seemed too severe to award either a 1 for a feature on the chance basis of just one occurrence of the feature in the data or an 0 on the chance basis that it had not occurred. For one grammatical variable, even, a threshold of 30% usage was arbitrarily set before a 1 was assigned. Fasold points out the obvious: Stolz and Bills have determined to treat certain variability as irrelevant. He also notes Labov's discovery that where stigmatized forms occur in higher class dialects, they occur at lower frequencies than where they occur in lower class

dialects. It seems wasteful of generalizations and insights to collapse information such as this into a binary rating. Fasold demonstrates how much more finely dialects can be graded through the application of that wasted variability. Next, turning to work done by Wolfram, he observes that grammatical variabiles were found there to stratify sharply across class boundaries. Thus frequency studies would seem particularly necessary to establish meaningful thresholds with grammatical variables--a fact consistent with Stolz and Bills' decision to set that 30% threshold for have got vs. have. In sum, Fasold presents a convincing argument that neither approach can afford to do without the other.

Since their initial proposals, Fasold's has not been the only attempt to find common grounds for implicational and variable analysis. DeCamp (1969) and Labov (1973b) admit they would prefer not to see their orientations continually viewed as irreconcilable. Bickerton (1971) and Wolfram (1973) identify the fundamental deterrent to a real merger of the theories correctly as incompatible positions on what Labov all along has called the 'inherent variability' of linguistic systems. In the introduction to a recent paper (1973a) on semantics, Labov claims linguistics has always been "the study of categories," but that such a study only becomes interesting--at whatever level, phonological, syntactic or semantic--when the boundaries between the categories occupy more of the linguist's attention than do the apparent categories themselves. The other orientation, while no less tough-minded is more humanistic. Rather than accept a certain amount of individual variation as inherent within and between categories, the other linguists would rather search for some social significance in that variation--something about the code or about its users. Theirs is a different philosophical perspective on what constitutes an important question in linguistics.

D. POLYLECTAL GRAMMARS

CHARLES-JAMES N. BAILEY (Technische Universität, Berlin 1969), another prominent creolist, would like to join aspects of linguistic analysis, previously considered incompatible, together to form a new theory of variation in language. He rejects (1969) two vital elements in the foundation of Saussurian linguistics--both the synchronic/diachronic dichotomy and the langue/parole distinction. The parsimony of Ockham again: for Bailey it seems time to admit the insights of historical linguistics and of dialectology into descriptive analysis. How can it be that a speaker can understand others from different regions and of different ages? If we are not limited to our own time and place in our use of language, why should the grammars we write be either?

Just as he asks some of the same questions as DeCamp, Bailey favors some of the same solutions. He imagines the spread of language change over a community in terms reminiscent

of the movement of a wave in physics. Temporal and idealized spatial parameters are both of importance, and changes seem to work their way at various speeds through various regions. Bailey proposes that the most essential task of a new linguistics freed of old constraints is to facilitate the writing of what he has called 'polylectal,' 'panlectal,' or more recently (1973), 'polysystematic' grammars. In this framework, methods of internal reconstruction and the comparative method would first isolate points of interest in the past and current state of the code. Then, the varieties to be differentiated on a Guttman scale would ideally be made to correspond in a hierarchical array to every possible combination of maintenance or shift of all the changes at work throughout the range of the speech community. Marking theory and frequency ratings--especially of the sort that Pasold has proposed--might also be used where appropriate.

The theoretical effects of Bailey's innovations would be great. Both idiolects and dialects would become fictitious, and all would speak what dialectologists have labeled a 'transitional dialect.' Still, Bailey (1973) feels that his theoretical position is essentially more conservative than those of some of his followers, and could be accomplished within the existing linguistic framework.

DEREK BICKERTON (The University of Hawaii 1971) stresses the individual in his studies of variation. Believing intragroup differences should not be glossed over, he joins Bailey in the writing of polylectal grammars. Not content to accept implicational scaling uncritically, still he (1973) accepts it as a useful tool for the formulation and testing of hypotheses. He also stresses the hierarchical and diachronic relationship implicit among the linguistic variables, envisages a continuum of varieties differentiated by single rules, and hopes that many of the feature dimensions corresponding to that continuum would order diachronically according to the appearance of new rules.

About the single most ambitious innovation in Bickerton's scalogram-based grammars would be the addition of a 'rule-shift component' (1971) to specify which rules of the grammar apply to which dialects. The base component, a special problem, would have to be optimally general making extensive use of semantic distinctive features and allowing for all of the specification of variation within the transformational rules.

The first NWAVE Conference, held in October of 1972 at Georgetown University was also the occasion of the Eighth Southeastern Conference on Linguistics. Sixty-four papers were presented at the combined conference, and of those, twenty-four relating (under various headings) to variation theory were published as a single volume, Bailey and Shuy 1973. Obviously, a short review paper can not do justice to the theories of all linguists working with variation today, but the positions we have presented touch upon the major issues addressed by all.

E. NONDISCRETE SYNTAX AND SEMANTICS

One more trend in current linguistic theory is not so much based on research into language diversity as it is relevant to it. A group of transformationalists, motivated by their own syntactic and semantic research have independently arrived at views that would extend variation theory from its original emphasis on phonology into the more abstract areas of 'semantax' (Shuy and Bailey 1973). In line with Labov's contention (1973a) that the boundaries of certain linguistic categories are difficult to establish, George Lakoff and John Robert Ross have, for several years, been publishing their findings of "fuzzy" or "squishy" linguistic data. They have found that the grammaticality of certain sentences will imply the grammaticality of others. (See Lakoff and Ross 1967; Lakoff 1970; and Ross 1973.) Even such accepted constructs as what Ross terms 'nouniness' will form an implicational array (1973:100) when decisions regarding the acceptability or nonacceptability of sentences are dependent upon them.

VI. OTHER TREATMENTS OF LANGUAGE DIVERSITY

It is difficult to supply some sort of unity to this last section with an easy and accurate clasification. As Haugen has observed, work with the actual data of language variation may justifiably be undertaken with no immediate intention of contributing to the development of theory either in the social sciences or in linguistics. Furthermore, practical research need be no less respectable than more theoretical research. Within this less formal area a few loose groupings do seem workable.

A. BILINGUALISM

Issues in bilingual research which were vital to Haugen and Weinreich decades ago are no less vital to Haugen and others today. Contrastive linguistics often leans more heavily toward psycholinguistics than toward sociolinguistics and has therefore largely been excluded from our treatment. Still some admirable work is being done with the social aspects of language interference in communities where bilingualism is widespread and well established.

WILLIAM F. MACKEY, the first Director of the International Centre for Research on Bilingualism, Laval University, has published extensively on bilingualism in theory and in practice. Whether it be in an abstract description of the use of two codes (1962), an analysis of methods in language teaching (1965), or a consideration of models for bilingual education (1970a), his work has displayed a typologizer's conviction that problems are best dealt with when they are understood in all their conceivable shapes. While the detail of his typologies may at first seem overwhelming and perhaps even unusable, Mackey has countered any such criticisms. In his opening remarks to a Unesco seminar assembled to describe and measure bilingualism, he has

argued we must know what we are measuring before the results will be of any possible use to anybody.

'Interference' has long been a theoretical construct in studies of the individual bilingual's problems of keeping his languages apart. At the 1970 Round Table at Georgetown University, Mackey (1970b) proposed a method for distinguishing interference from the 'integration' of elements within the code of the bilingual community as a whole. He reasons that every time a bilingual departs from the monolingual norms corresponding to one of his languages and appears randomly to have mixed in an element from the other, the speaker may not in fact have made a mistake. Sometimes what originally were the mistakes of elders are passed on to their children. The variants become standard items in the speech of succeeding generations. And sometimes bilinguals in conversation with other bilinguals just enjoy playing games with their languages. Working with French-English bilinguals, Mackey compared huge word lists, ingeniously elicited, in an attempt to establish local norms before classifying specific occurrences of mixture either as the result of interference or integration.

Although more recent work on code-switching (for example, that of Gumperz) would suggest that she should have considered a greater range of stylistic variables, JOAN RUBIN, Tulane University, has urged caution in accepting Ferguson's notion of diglossia. Rubin (1968) surveyed the comparative use of Spanish versus Guaraní, a native Indian language, in three selected locales in Paraguay. An entire rural village, Luque, as well as Itapuami, the country's fifth largest city, and Asunción, the artistic and governmental center, were canvassed. Emphasis, though, was on the first two locales. Questionnaires, mostly orally administered by the investigator, were designed to isolate specific situations in which use of each language predominated.

The most powerful variables turned out to be scales of rural/urban and formal/informal, with these in turn being skewed by individual factors of language competence, first language acquisition and education. When viewed closely, Rubin concludes, the use of Spanish and Guaraní seem more to be in something like free variation than in complementary distribution, and hence far more complex than previously supposed.

The Cross-Cultural Southwest Ethnic Study Center, co-directed by JACOB ORNSTEIN, involves faculty in the Departments of Sociology, Linguistics, and Education at the University of Texas, El Paso. For some time now, Ornstein and his colleagues have been carrying out a detailed study of local Southwestern bilingualism. Contending that no university in the United States could be more representative of the norms of the bilingual/bicultural community that surrounds it, the researchers have drawn a stratified sample of the entire undergraduate enrollment, Mexican-American and Anglo alike, at UTEP. They have paid careful attention to such variables as age, sex, year of studies, and others. A 106-item Sociolinguistic Background Questionnaire requests demographic, bilingual-usage patterns and language at-

titudes information, in addition to data on life styles and other culturally relevant details. Part B is an open-ended interview set up to get at proficiency in both English and Spanish as well as recognizable code-switching varieties. This is followed by the writing of three compositions at elementary, intermediate and advanced levels of difficulty. As much as possible, the written sections stress topics of interest to border youths, and, so that some control of comparative performance ability can be maintained, identical themes must be chosen for both languages. Finally, all students are asked to fill out Pace's College and University Environment Scales (CUES), a 140-odd, multiple-choice test designed to determine subjects' perceptions of their school environment.

While strong conclusions await replication, Ornstein and Goodman (1974) report English performance to correlate strongly with both Spanish performance and grade point average. But less predictably, and unfortunately, the Chicano students seemed unrealistically critical of their own skills both in English and Spanish. They displayed a simultaneous loyalty to their cultural-linguistic traditions and a tendency to rate these very characteristics as inferior to their dominant-culture counterparts. Such findings are, of course, quite in line with those of Lambert and Gardner (in several of their essays, 1972), Cohen (1975) and many other authors, causing Ornstein and Goodman to wonder whether these poor self-images might be comparable to trends within minorities of the Third World countries.

B. LANGUAGE PLANNING

In our last survey we specifically apologized for ignoring certain areas of sociolinguistics in which the language diversity issue *per se* was not primary. That policy is still best, but this time we wish to recognize how closely dependent some research areas are to the preexistence of reliable information on the status each code might have in a mixed community. Language planning is such an area. Three scholars already prominent in our essay are perhaps more closely associated than any others with this field. Einar Haugen has published on the theory and practice of planning with particular emphasis again on Norwegian. Fishman has edited and coedited collections relating to these matters as well as reporting on his own work on planning in the linguistically diverse state of Israel. Finally, Joan Rubin has coedited two separate anthologies on the subject.

Fortunately, Joan Rubin has recently contributed an excellent annotated bibliography (1974) of selected titles in language planning to the newsletter of the Center for Applied Linguistics. She discusses the basic issues involved in planning characterizing activities in the field as the "conscious organized efforts to solve a community's language problems by governmental bodies, with attempts to consider alternatives and forecast outcomes" (7). Clearly, good solutions and forecasts depend on good understandings of real problems. Diversity studies are not to be slighted.

We advise our readers to consult Rubin's bibliography for titles of and comments on the most important work in this area of what is decided macrosociolinguistics.

C. BILINGUAL EDUCATION

Most of our readers will know that an enormous amount has been written on what bilingual education should be, is, and is not. This survey is no treatise on bilingual education, and we have no intention of getting into any of those controversies. What we would like to do, though, is suggest a few of the ways sociolinguists have tied their better understanding of communities to proposals for better education in them.

By the end of the 19th century, prospects looked dim for the maintenance of the national language in Ireland. Then, in this century, concerned patriots did a bit of language planning, lobbying to pledge the nation's schools to revive Irish, teach it as a second language, and employ it as a medium of instruction. After forty years of this arrangement, JOHN MACNAMARA, now of McGill University, determined that Irish children were spending something like forty-two percent of their schoolday learning Irish. He wondered to what avail.

His Bilingualism and Primary Education (1966) became the first major investigation into the effects of the Irish revival. But it has become a standard reference work in considerations of language policy and public commitment to bilingual education as well. Trained as a psychologist, Macnamara realized the value of careful sampling and testing. His sample included over 1,000 primary school children drawn from better than 100 Irish schools (mainly in English-predominant areas). The children were thoroughly tested for proficiency in both the English and Irish languages with commercial examinations as well as others of the investigator's devising. Another battery of instruments measured proficiency in arithmetic skills--both mechanical and problem-based.

In general, Macnamara's conclusions seem rather gloomy regarding the efficacy of an imperfectly learned second language for instructional purpose. He found the Irish children notably behind their peers in England in the ability to solve arithmetic problems. Furthermore, he pointed to a 'balance effect,' mentioned by others in the literature on bilingualism, suggesting that the effort children spend learning a second language may rob them of their full development in their original language. The net effect of pushing for second language mastery to the exclusion (or near exclusion) of first language training may be that large numbers of children leave school below their monolingual potential in either of their languages.

Upon superficial acquaintance, the result of the more recent St. Lambert Experiment seem as optimistic for the good effects of a second language as the medium of instruction in grade school as Macnamara's were pessimistic. Wallace E. Lambert and G. Richard Tucker (1972) offer a word of caution, however,

that one should not lightly equate one bilingual situation with another. It is highly possible that the nature of community attitudes toward the languages involved can contribute greatly toward the generally discouraging result observed by Macnamara or the encouraging ones reported by Lambert and Tucker. Nor would it be smart to assume that the viability of other--perhaps more complex--models of bilingual education have been undermined when compared to the results that have emerged in Montreal.

In broad outline, the St. Lambert (Montreal) experiment has been simple enough. A special class was established to allow children of willing parents to study exlusively in French in an otherwise all-English primary school. Their progress has been carefully monitored through their first seven years of school and compared on a range of scales and criteria with that of an all-English control group (Bruck, Lambert and Tucker 1974). The assumption--perhaps too naive--has been that whatever differences may be observed have resulted from the different treatment which the experimental group has received. There have been, too, some remarkable differences and no major disappointments. The experimental group have mastered French at a very high (but not native) level, they have shown no real signs of retardation in subject-matter skills, their attitudes toward French Canadians have been considerably more favorable than those of the controls, and they have given a few encouraging signs that a Vygotsky-like transfer of skills across languages seems to be emerging.

Service on the staffs of two separate bilingual education programs in California has provided ANDREW D. COHEN (UCLA) some interesting insights into what might be generalizable from the St. Lambert experiment. In Redwood City, California, he worked (1973) with 90 Mexican-American children, 45 taught bilingually and the remainer traditionally in English only. The children receiving the bilingual instruction were further divided into three sub-groups, but they all were taught along with Anglo classmates in both English and Spanish. (So it is already clear that this was a different sort of "bilingual education" from that described in the St. Lambert experiment.)

Tests were selected or designed to measure language skills in both languages in listening, speaking, reading and writing. Questionnaires regarding language use and attitudes as well as the usual academic aptitude instruments were also used. After careful pretesting and posttesting over a two-year period, Cohen (1973) proposed some cautious generalizations to an international seminar on language testing. Much of the research design, based on Fishman's Barrio project, had proved problematical for one reason or another. Some of the hardest problems still involved elicitation techniques and the selection of actual stimuli to get normal, representative speech from children who find themselves suddenly in the strange sociolinguistic situation of testing. Cohen had experimented with a few attempts to encourage and measure switching and to measure the facility to

describe events pictured in various media, but felt much more would eventually have to be done. Setting up objective criteria for rating subjective reactions to children's performance was a headache. Cohen felt he had learned a good deal, but much of it was about the ignorance of the profession. It took his work on the Culver City project to suggest to him why the Redwood City children had not done as well as an idealist might have wished.

Cohen (1974) regrets that in too many bilingual education projects Anglos are either not included or simply "tag along." The Culver City children have responded differently. A Pilot group of 19 five-year-old monolingual English speakers have been taught as in the St. Lambert project--monolingually in the language of the minority culture from kindergarten on.

Looking back and comparing, Cohen concludes the Redwood City dual-media approach had not really made the Anglo children fluent Spanish speakers or even significantly appreciative of the Mexican-American heritage. The Mexican-American children had fared no better. There were too few incentives for either of them to put out real effort to be bilingual. For the Anglos, English was ultimately available when needed, and all knew that English remained the language of prestige. With different ground rules, the results in Culver City underscore the lessons of the success in Montreal. Even with less enthusiastic initial support from some sectors of the community, the California results quickly resembled the Canadian. Comparison with another Anglo group as well as a group of Ecuadorian children raised and taught monolingually in a Hispanic environment generally indicate skills that compare favorably in both languages and in various subject matters.

What these successes in the less usual bilingual education of dominant-culture children might necessarily mean for the education of minority children is not obvious. Still the implications are strong that the improvement of educational programs will depend in part upon an increased sensitivity to sociolinguistic variables. The social, psychological, and linguistic variables hampering the education of American Indian groups are coming increasingly to the attention of educators, government officials and concerned citizens. But while the cultural conflicts between the dominant culture, its educational patterns and the traditional ways of these particular minority students may be more widely respected than they once were, they still are far from well understood. In the late 1960's reports on the success of an experiment in the bilingual instruction of Mexican Indian children brought the speculation from educators in this country that the same methods might work here.

NANCY MODIANO (1968) reports that her efforts to teach children from a Highland Mexican tribe to read first in their native language and only later to read in the national language (Spanish) were a remarkable success. When compared with a control group taught to read in Spanish from the beginning, the experimental group quickly showed better reading comprehension <u>even in Spanish</u>. Modiano's explanation has been that learning

to read is a sufficient challenge in a code a child knows well. When both the reading task and the language are unknown, the problems are compounded. She suggests that national language oral skills be developed as reading skills are progressing in the minority language so that the eventual transfer can be as productive as possible. Her present affiliation is with the Instituto Nacional Indigenista de Mexico.

Shortly after the initial publications of Modiano's results, BERNARD SPOLSKY established the Navajo Reading Study at The University of New Mexico partially to consider the feasibility of a replication of the Mexican experiment with an Indian group here. Wary of inflated claims of the intrinsic value of any particular methodology for language teaching over any other, Spolsky set out to understand the Navajo community, Navajo language and Navajo education as well as possible before deciding any method might be best for their needs. He recently has referred (1974:17) to this conservative attention to all known sides of linguistic-pedagogical issues as 'educational linguistics.' In the half-dozen years of its existence, the Navajo Reading Study has been instrumental in bringing a wide range of educators, linguists, bureaucrats, social scientists, and tribesmen together to work on issues that have ranged from the description of the Indian language, its speakers' reactions to it, and its place in their lives to materials development and teacher training. (See Spolsky 1972, "Introduction," and Spolsky 1974).

D. BLACK ENGLISH

What sense does it make for us to throw a topic like Black English together with bilingualism, language planning and bilingual education? Why is it in an article on language diversity at all? The questions are old ones. Walt Wolfram (1974) reviews some of the literature on black and white speech differences and identifies someone at just about any position on a continuum ranging from varieties of black and white speech are "virtually identical" to the varieties are "far from identical." There even have been proposals that the task facing the ghetto child who must deal with the language and culture of the white majority is sociolinguistically more difficult than that of the child who speaks a language that everybody knows is completely different from English. In many ways the issue of Black English and its place in society could not have better companions in a review article than it has here: we are dealing with something very much like bilingualism, our society will have to do something very like language planning, and some resulting school programs are likely to be very close to bilingual education.

The strongest influence bringing the term 'Black English' to public attention has been J.L. DILLARD's (University of Puerto Rico) bestselling success with his Black English: Its History and Usage in the United States. As suggested in the subtitle, the author leans heavily toward a historical

presentation. He is a creolist of long standing and has taken the position--shared by other creolists, principally William A. Stewart--that rather than having developed from some British dialect, BE has an African heritage. The proponents of this 'decreolization hypothesis' speculate that the origins of the speech of today's Blacks were in a pidgin (of a nature none too clearly defined) which moved through a creole stage and is now reversing the process as it becomes more like Standard English.

The general American reading public took well to Dillard's book, as had the reviewers of the national media. It was not, however, an unqualified success with sociolinguists. In a harsh review in Language, Wolfram (1973c) points out how well it is known that the observer untrained in linguistics will often overreact to the speech of others, attributing to it an exaggerated frequency of disapproved items and surely not describing it accurately. And yet Dillard's main source of examples of the speech of slaves is drawn from novels and such written in the Plantation Days. Nor does Wolfram draw the line at doubting the accuracy of Dillard's second-hand observations. Even with all of that, Wolfram pays his respect to Dillard for having brought a new popularity to linguistics in general and sociolinguistics in particular. The news of the naturalness, social utility, and rule-governed qualities of diverse speech is starting to spread.

After all, says Wolfram, Dillard is "no Johnny-come-lately on the bandwagon of interest which BE is currently enjoying" (670). Neither are Wolfram or a number of his close associates new to the movement.

Much of the 1960's impetus for linguists to work harder on better descriptions of Black English results from their professionally and humanistically offended reactions to the decade's Head Start Programs. In the fullest statement of their beliefs, Carl Bereiter and Siegfried Engelmann (1966), the spiritual fathers of Project Head Start, proposed that the most crucial deprivation of all to ghetto children is their 'verbal deprivation.' They claim slum children lag behind their more advantaged middle class peers in the size of their vocabularies, length of their sentences, and general complexity of their grammar. (The temptation was strong to write "grammars," but that is not what B&E meant.) But most damning of all, these youngsters apparently lack the verbal skills that would support and encourage the kinds of reasoning abilities necessary for school work. These deficits more than any others--the B&E model is often referred to as the 'deficit' model--are said to keep such children down. They fall farther and farther behind. The architects of Project Head Start reasoned that only dramatic preschool intervention programs aimed at overcoming their greatest failing--poor language skills--could bring school success to these children. The results have not made believers of many sociolinguists.

Earlier, in the section on sociological theories of language, we promised to return to the ideas of Basil Bernstein. Largely because his writings are so often abstract and sometimes vague, Bernstein has been cited as the guru of those of

disparate persuasions. It is not likely that Bernstein could be all things to himself even though he has seemed so to so many. Actually, Bernstein has been cautious in projecting whatever implications his work may have for education. We refer the reader to a fairly recent article (Bernstein 1972) in which he argues against the straight-jacketing implications others have seemed possessed to find in his mere reference to the characteristic speech of most poorer children as 'restricted.' He maintains he is observing tendencies of social context, patterns not straight-jackets. He states emphatically (146): "Because a code is restricted it does not mean that a child is nonverbal, nor is he in the technical sense linguistically deprived." Whatever implications he sees for educators in his theories are largely in the area of teacher training and community education (149): "If the culture of the teacher is to become part of the consciousness of the child, then the culture of the child must first be in the consciousness of the teacher."

Bernstein may have been responding directly to William Labov in refuting any claims he had attributed a hopelessly "nonverbal" character or a real state of "linguistic deprivation" to lower-class children. Labov is as good an evangelist as he is a linguist. Head Start and the Bereiters and Engelmanns--whether or not they had misformed their ideas from Bernstein--had so outraged him that he responded in kind to their fervor. In a Georgetown Round Table presentation in 1968, he brought the house down with "The Logic of Nonstandard English." Our purpose in this essay is to report what linguists have had to say recently about language diversity; Labov's point in this much-anthologized paper is that diversity is just diversity and not perversity. If Bereiter and Engelmann's model can be called the deficit model, Labov's is the 'difference' model. A speaker of a divergent dialect can easily be verbal and capable of thinking. What he appears not to know is not language, but the standard dialect.

It seems that work on Black English divides readily into the traditional areas of all linguistic research. We have already seen that sociolinguistic research can take two main turns --to emphasize the sociological or the linguistic. These are the "pure" concerns. If a linguist ventures out of the pure and into the "applied," he generally gets into contrastive analysis. And if education is his interest, he proposes teaching models based on all of his sociological, linguistic, descriptive and contrastive theories. That is exactly the picture in studies of Black English.

Labov may not be the only linguist to have done all of the above with Black English. But he is the one who comes most readily to mind as having done them all with such distinction. The University of Pennsylvania Press has recently published a representative collection of his essays on all aspects of Nonstandard English (Labov 1972). The essays are grouped into three categories. The first, "The Structure of the Black English Vernacular," includes his major papers on variation theory

--the variable rule, the notion of inherent variability, etc. Parts two and three are more sociological, attempting to classify his remarks either as relevant to the "social setting" or "uses" of the vernacular. In fact, all of the papers in each section have something of the flavor of the others. Educational implications are explored wherever appropriate whatever the classification.

It is convenient to discuss the work done on Black English by Walt Wolfram and Ralph Fasold together. They have frequently collaborated and seem in basic agreement on most issues. The design of the Detroit Dialect Survey (Shuy, Wolfram and Riley 1967) was largely drawn from Labov's study of the Lower East Side of New York City. This accounts for Wolfram's early commitment to firmly sociologically based dialect work. Both the findings of the Dialect Negro Speech often read like Labov displaced to Detroit. Often but not always.

R. K. S. Macauley's trenchant review (1970) of Wolfram (1969) calls much of the methodology, not only of Wolfram alone but of the whole Detroit project, into question. The arguments are detailed, but essentially they bear on deficiencies in sampling, lack of controls for stylistic variation and insufficient attention to subjective reactions to speech. These criticisms are not proof that the Detroit study was defective. They do suggest that a respected model would not assure respected results if corners really were cut. Many accurate and painstaking replications must precede a new model's acceptance.

More recently, Wolfram's work has blended a few new concerns with his more standard ones. In Wolfram (1974) he deals with the influence of Black English upon the English of second-generation Puerto Ricans in East Harlem. The sampling is more convenient than scientific. Twenty-nine males, all members of a local youth group, were interviewed. The effects of stylistic changes were not of great concern. What the author calls "general sociolinguistic principles" (222) were what he was after. The features of bilingualism in a contact situation involving two minorities pose a real challenge to any sociolinguistic methodology. Ockham's razor: is it better to be very general or very specific? From the standpoint of doing a descriptive study, Wolfram purposely chooses the general. Even with the rules he writes he prefers the general. Rather than to build percentage statements into variable rules as Labov has proposed, he would rather find a few easily generalizable linguistic contraints, rank them in order, order the rules, and try to handle as much variation that way as possible.

There seems to be a significant shift in perspective in Wolfram (1974b). The topic involves differences between Vernacular Black English as it is presently understood by linguists and the less-well-documented speech of southern whites. In analyzing these results, he begins to sound more like Bickerton and Bailey than like Labov. He places an emphasis on the speech styles of individuals and tries to work out something of

the origins and development of the English spoken by poorer whites in the South today. Once impatient (Wolfram 1973c) with Dillard's literary examples in support of decreolization, he is now using them himself. Once less concerned with individual than with group performance, he now includes implicational arrays of his data and speaks of rules to specify various 'lects.' More typically, he has proposed a theory of 'selective adoption.' Some grammatical borrowings relate to lower-level rules, are more easily accommodated into a borrower's grammar, and thus will be taken up. This relates well to the publications of a scholar who has consistently distinguished between the 'gradient' variables that structure tightly into the speech of a whole community and the 'sharp' ones that can be more consciously responded to and thus function to maintain group distinctions.

Ralph W. Fasold (1972) conducted his own study of Black English in a metropolitan area--in this case, Washington, D. C. Again, the methodological model was Labov's, and again Macaulay (1974) came down hard upon the results in a review in Language. This time the reviewer was not so specifically critical of the sampling procedures as of the choice of population to be sampled in the first place. Macaulay obviously believes a report says little about a community unless it says something about the entire community in all of its phases and functions. A study of the interaction of Black and Standard English must be based upon more than a sample of blacks. Fasold obviously believes otherwise.

Macaulay also disapproves of Fasold's format for writing variable rules, and this brings up an essential similarity that makes Wolfram and Fasold's work belong in the same discussion. When matters of sampling and analysis come simultaneously into such critical scrutiny, the issue is neither squarely linguistic nor squarely sociological. This is the sort of situation that prompted us to label some work "intermediate" in our last survey.

We have referred to Macaulay's review of Wolfram and Fasold to sample some serious, divergent opinion in the field. We took no sides. We cannot agree with another opinion held by Macaulay; he charges that the Urban Language Series, published by the Center for Applied Linguistics, reflects a narrow editorial policy. He notes that since the appearance of Labov's Social Stratification of English in New York City in 1966, only seven (now eight) subsequent volumes have come out and that the same names have appeared again and again as authors or editors. Either General Editor Roger Shuy plays favorites or he has too few choices, Macaulay concludes. Perhaps it does indicate the latter, and there is no doubt that these writers (see Wolfram, 1973b, for a list) form a sort of "school" of linguistics (we referred to it before as the "Washington, D.C. School"). Be all that as it may, Shuy and the CAL are providing a service to us all making these works available.

Before moving away from Wolfram and Fasold, we should mention two articles they have written jointly and directed toward in-service or prospective teachers in the inner cities. Each

of the essays first appeared in issues of the CAL Urban Language Series: "Toward Reading Materials for Speakers of Black English" (1969) and "Some Linguistic Features of Negro Dialect" (1970). Both have been anthologized elsewhere to bring the ideas of the sociolinguists to educators.

Formerly based in Washington, D.C. as well, but now in New York City, WILLIAM A. STEWART has been the most outspoken linguist to work in the field of second dialect teaching. Members (past or present) of this "Washington School" do not always agree on linguistic interpretations--for example, Stewart's decreolization hypothesis--nor do they always favor the same solutions to teaching problems. Joan Baratz and Stewart are closely identified with proposals for the development of basal readers in the nonstandard vernacular. The lines of their arguments harken back to the Modiano proposal for bilingual education. Stewart is also indispensable to any collection of essays on Black English.

While they have their differences, all of the linguists discussed so far in this section agree in principle that black school children should be encouraged to keep their ethnic variety of speech and simply add Standard English beside it. "Simply" is actually a poorly chosen word, for education in this sort of sense of appropriateness, as well as the development of requisite motivations to accomplish the learning, are not simple at all. Some say they are not even worthwhile or moral. All linguists are not bidialectalists. JAMES SLEDD, of the University of Texas (Austin), is a most (and probably the most) articulate spokesman of the fairly new minority position. The titles of his best known articles on the subject are a clue to the wit of their author: "Bi-Dialectalism: The Linguistics of White Supremacy" (1969) and "Doublespeak: Dialectology in the Service of Big Brother" (1972).

Sledd is right on a number of points. The best descriptions of Black English so far produced are not really up to the demands of supporting a valuable contrastive analysis of the nonstandard and standard varieties of English. (We probably don't know enough about Standard English either.) And what proof is there really that languagemakes all that much difference in determining the plight of American Blacks? Sledd is a historian of the language, and he believes social change generally comes before--not after--language change. The issue is not communication either. The issue is prejudice. As hard as sociolinguists are working to reconcile their theories of language and society, Sledd throws the challenge at them of making an additional reconciliation that may prove even more difficult: their profession with their consciences. Do linguistics and cultural pluralism mean that communities accommodate more than one language and culture or that individuals are required to be pluralistic to suit other individuals?

We cannot hope to offer here a complete survey of Black English, the education of blacks, and all related issues. In-

terested readers will probably want to look through some of the articles in the following anthologies: Abrahams and Troike 1972, Bailey and Robinson 1973, Baratz and Shuy 1969, Bentley and Crawford 1973, Cazden, John and Hymes 1972, DeStefano 1973, Fasold and Shuy 1970, Shores 1972, and Williams 1970. All of these collections do not treat Black English from the same perspective. In some instances the emphasis is on the education of all minorities, in others language variation and change, and others specialize in issues exclusively Black. Regardless, one will soon note a fairly small set of contributors appearing again and again--often with the same essays.

For a short, readable account of all the major issues relating to Black English as a sociolinguist sees it, English in Black and White, by Robbins Burling (1973) is our recommendation.

VII. SOME CONCLUDING REMARKS

Survey papers generally reach their conclusions as they go along, and about all that remains to be done at the end is to think of a way to say there is little left to say. Still a sense of symmetry calls for more than that. Throughout this survey we have attributed to sociolinguists a maverick sort of perspective on language study, an impatience with prefabricated walls around disciplines. They size up complex problems, estimate what they think they can handle, and then try to come up with the machinery to do the job.

In the survey we had to size things up ourselves. A great deal of the research we had to exclude as either too minutely micro or too grandly macro is fascinating research. We must admit the use of "partial" in our subtitle has been accurate in all three of the meanings in Webster's Seventh New Collegiate Dictionary: it is "biased" and favors direct work with language variation over some other sociolinguistic concerns; it shows we are "markedly" (but hopefully not "foolishly") impressed by the work we have included; and we do realize there is more to sociolinguistics than this.

A reader already familiar with most of this material can evaluate our evaluations. Others may have become interested in something new and may appreciate the following advice. The new NWAVE Conferences at Georgetown University will bear watching. Fortunately, ROGER W. SHUY is there directing the Sociolinguistics Program at GU as well as the Domestic Program of the Center for Applied Linguistics. In addition to the first (and soon second) set of NWAVE papers, Shuy has either edited or coedited a number of other collections published by the Georgetown University Press. He also has the Detroit Dialect Survey and the CAL Urban Language Series to his credit. Somehow he finds time to write, and we especially recommend Shuy (1974) and Shuy (in press) as good follow-up reading to our own survey. The titles, "What Is the Study of Variation Useful For?" and "Breaking Into and Out of Linguistics"

suggest their practical, readable and thoroughly interdisciplinary characteristics.

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THE STRATEGIES OF LANGUAGE USE

Robert J. Di Pietro
Georgetown University

There are many ways to define the pursuits of linguists so that they fall within boundaries acceptable to the field in general. Perhaps the most obvious criterion would be to insist that the focus of the investigator's work be on some aspect of human language. In recent decades this focus has fallen almost without exception on the grammatical artifact of language. It is not hard to imagine that future historians of linguistics will look back at the past twenty years of our professional existence as the great age of grammar. The impetus given to grammar studies by Noam Chomsky in the late fifties soon made any other pursuit subsidiary to that of devising grammatical statements for language. Recalling a definition given by Chomsky and Halle, namely that "a grammar is a theory of a language" (see Preface, p. ix, The Sound Pattern of English, Harper & Row, N. Y., 1968), one can easily understand that, for anyone willing to accept such a definition, whatever is worth discovering about language should become part of its grammar.

That we have been living in the age of grammar is further borne out by the kind of work which has had some success in drawing attention away from transformational generativism during recent times. The challenges to the hegemony of the generativists have come forth as alternative grammatical theories. Among the most notable is tagmemics whose adherents claim advantages in formulating the grammars of unknown languages. There is also Sydney Lamb, with his stratificational approach. The multi-layered orientation of those linguists who follow Lamb is seen by them to provide for a greater comprehensiveness of statement. Fillmore's case grammar and Chafe's unidirectional model are among the latest to arrive on the scene. Whatever the model, however, intergroup debate appears to center largely on the claims made to explain the many forms taken by utterances in a language.

If there is any real alternative for linguists to formulating grammars, what would it be? My suggestion is that we concern ourselves directly with the use of language. Surely, the ways in which people use language to enter into communication with each other, how situational roles are discerned in interaction and how specific personality types are portrayed via language are all matters meriting investigation by professional linguists.

Some work has already been done in the above-mentioned areas of language use, but it has been primarily by scholars who would not consider themselves linguists. Here I can refer to the achievements of Erving Goffman, the sociologist for his articulation of frame analysis, as well as to Dell Hymes, the anthropologist, and John Gumperz, the ethnographer, for their work in the structure of conversation. Among the linguists who have come to concern themselves with language use are the so-called pragmatists. These linguists have recognized that the use of a sentence in a given context ultimately affects both its form and its meaning (see P. Griffen, "Linguistic Terminology", The Linguistic Reporter, vol. 16, no. 9, Nov. 1974, p. 2, for a brief, but clear discussion of pragmatics).

While it is important to consider the part played in sentence generation by context, intention, and shared information, such an approach is still essentially artifactual. It should be equally legitimate for linguists to shift away from utterance form to the dynamics of interaction. How are utterances used strategically to achieve desired goals? What are the ways in which the choices of particular expressions motivate the participants in speech acts to either abandon desired goals or feel that they have been reached? To understand the workings of these choices requires a shift from a grammatical focus to a communicational one.

At this point, some definitions are needed. I have been talking about grammatical artifact. Grammar is, indeed, an artifact. It is made by humans and institutionalized by them to do things that are often quite independent of the communication of information. William Labov & the sociolinguists who study variation in language have shown us how people can portray the patterning of their societal structure by tagging certain aspects of their grammars as 'socially variable'. Nation States, to offer another application of language artifacts, have used grammars through history to consolidate their populations. In this context recall the remark made by Elio Antonio de

Nebrija, the first grammarian of the Spanish language. The year was 1492 and he had just presented the Spanish queen with the first copy of his Gramática castellana. Her question to him was potentially disarming: What good is your grammar? What do you do with it? Fortuneately for grammarians everywhere, Nebrija had just the right answer. You need it, he explained, to assure that all peoples subjected to the Spanish crown will have one voice with which to talk to the Queen. Besides, he continued, 'siempre la lengua fue compañera del imperio'--language has always been the companion of empire.

Today, developmental psychologists look for the use of certain grammatical forms as signs that children are maturing cognitively. The metaphor they use is the "LAD", the "Language Acquisition Device"--that little black box which is owned by each and every human child and from which arise all sorts of sentences.

Last but not least among the utilizers of grammar are the language teachers who equate the learning of a new language to the acquisition of the grammar of that language. Even the dialogs supplied in our audiolingual texts of today are to serve the ends of mastering certain grammatical patterns or rules. There has been no attempt to present the speaking of, say, French by showing how it would be used to express universally shared human goals in a French setting. Such an approach would not be artifactual. It would not focus on the forms of grammar used but rather on how the language can be manipulated to gain certain ends. In short, the approach would be strategic.

I believe that there are verbal strategies in language which can be identified and labeled just as one might identify and label grammatical constructions. They have, as their purpose, the attainment of success in dealing with other human beings. Among other things, they have to do with avoiding accusations of guilt, seducing, misleading, excusing oneself for a transgression, and making disclaimers. They can be drawn from several sources: live interchanges, plays, operas, novels, and other literary genres based on language. The source I have chosen here is that of children's language. The following examples have been taken from the speech of my two children, Angela, age 6 1/2 years, and Mark, age 5. The strategies were employed at different ages.

(1) Dependency Posturing. My daughter, by the time she reached three years of age, had changed her way of addressing me from Da-Da to her present form, Daddy.

Shortly after making this switch, however, she decided to engage in some feigned 'baby talk' with me. Her purpose was to curry favor and the strategy she used was to sound 'cute'. How does a child sound cute? Angela's way was to regress to an earlier linguistic stage which she could still remember. She began to drop articles and third person endings. Most notably, she began to call me Da-Da again. Even now, at age 6 1/2, she occasionally uses this form for the same purposes. Angela's awareness of the strategic value of 'baby talk' stems from her understanding that a posture of dependency upon an adult is likely to result in achieving a desired favor. However, this particular strategy involves much more of potential interest to the linguist. It involves an association in the child's mind between language and developmental history. The child is not only aware that the artifact of her own language has changed but that regression to an earlier stage of it can signal dependency.

(2) The Metonymic Cushion. On another occasion, at age 5, both she and my son Mark were engaging in some rough play with me. At one point, I feigned injury (after an unusually sharp kick in the solar plexis). Grimacing with pain, I professed to know which one of the two delivered the kick and therefore should be punished. Angela, showing some concern for my hurt, asked, "Was it December or June?". In this particular use of metonymy, my daughter was referring to the months of her and my son's birthdays. 'December' represented her and 'June' was Mark. There are many observations to be made here. First of all, metonymy, that is, the referral to one thing by mentioning some other thing associated with it, can provide a social 'cushion'. It helps to insulate the person referred to from the action of the instance. The reference to birthdays is especially effective to a parent who is then led to recall the yearly anniversaries of the birth of his offspring and thus to remember his parental role. While Angela was concerned for me, she chose not to indict herself for a bad action. Instead, she used language to remind me that I was the parent and they were the children. I should be the one to take the chief blame for what happened.

(3) The Reverse Double Bind. The third illustration of a child's verbal strategy is the one in which the child attempts to place the parent (or adult) in a 'double bind'. The term 'double bind' has been used to label the situation where the child inadvertently places himself in a position of anxiety. For example, incessant petitioning of the parent to gain a favor is likely to place the

parent in a mood not to give the child anything. My son first used a reverse double bind at around age 4. He initiated it by hurling a challenge at me: "I bet I know what you'll say if I ask you something". The semantics of the utterance forewarn the adult that a request is on the way which normally elicits a negative response. The invitation is out to play a verbal match-game and the child has made the first move. Sensing that a request is coming, the adult is led to choose between two possible retorts: "No, you don't know what I'll say" or "Yes, you do". The child is then free to make his request. In this case, it was to play a seemingly endless and frustrating game called Candyland. If I said "no" to his request, he could win the verbal match by saying, "See, I knew you would say that.". If I said "yes", he would then lose the match but gain a greater victory in getting his request approved by me. Since his initial motivation was to get me to play the game with him, he really gives himself only two choices: either win big or win small. He can't really lose.

The three verbal strategies illustrated above may have been used by children, but I believe that childhood interactions become the staging ground for the more mature strategies that will come in later life. As adults, other, more subtle ways will be used to display feigned defenselessness. Other ways will be added to metonymy to express concern about an event without implicating oneself deeply in the causes of it. In fact, children learn rather early the value of impersonal and passive sentences for the same purpose. It got broke is a much more non-committal answer to Mommy's question about the vase found in pieces on the living-room floor than any sentence expressing an agent, even an unknown one as in Somebody broke it. Placing a person in a double-bind, as by vocally second-guessing their response to a yet-to-be-posed question eventually becomes one of the cornerstones of adult oneupmanship.

The significance of strategies for achieving fluency in any language is reflected in the set expressions each language has to signal them. I call those strategies to which are assigned set expressions 'speech protocols'. One such protocol is the squelch. In English it is signalled by the expression, I told you so. In German, it comes out, Was ich immer sag' and in Italian, te l'avevo detto. It is used by speakers of all three languages to reassert a role of wise councelor. Another characteristic of speech protocols is their affinity for specified situations. Thus, Excuse me is used in English either as an apology for bumping into someone or

for moving them out of your way. In Italian, however, a distinction must be made between Permesso said to move people aside and Scusi which is used to express regret at making body contact with someone. Contrary to what textbooks usually say, adiós in Spanish does not mean just 'Goodbye' but also "Hi" when passing an acquaintance without intending to stop and start a conversation.

In concluding, I would like to suggest a way for linguists to make their approach to language more communicational. It is simply to collect strategies and protocols, find some kind of label for them, and then build a theory of verbal interaction around them. If nothing else, it is interesting work and should keep us busy for some time to come.

ETIQUETTE BOOKS AS LINGUISTIC AUTHORITY

Connie C. Eble

University of North Carolina at Chapel Hill

The critical reception of Webster's Third New International Dictionary in 1961 showed that educated Americans still welcomed authority in linguistic matters. People who readily accepted the principle of cultural relativity and who were willing to admit areas of gray in questions of morality and religion were outraged that the dictionary no longer offered a simple right or wrong verdict in matters of spelling, pronunciation, and usage. Such linguistic insecurity is the product of a long American tradition of authoritarianism in language use and is the price paid for a society formed on class mobility and ethnic diversity.

Bolinger (1975, 567) distinguishes two types of linguistic authority, informal and formal. Informal authority is largely unconscious and consists of the pressures or models that cause a speaker to identify with and conform to a particular type of language behavior. Formal authority is conscious and based on rules which delimit correct and incorrect in the absence of comfortable and daily contact with speakers of the desired language variety. Formal authority is the means for deliberately acquiring new skills in language use.

An interest in language and a desire to conform to the speech of prestigeous members of society became the concern of many middle-class Englishmen during the Renaissance. The changing social and political order of the sixteenth and seventeenth centuries gave the middle-class unprecedented opportunities for material prosperity. To aid and hasten their upward progress they sought to acquire new social skills, creating a market suited to authoritarian pronouncements about all facets of social interaction, including language. (Wright, 1935, 128) The humanistic and often pious courtesy books of the Renaissance eventually developed into etiquette books based on the social usage of the elite. Beginning in the eighteenth century, dictionaries and prescriptive grammars became the primary sources of linguistic authority, although the courtesy/etiquette book still devoted many pages to polite conversation and continued to be a source of formal authority. This paper is a historical study of the contribution of courtesy/etiquette books to the shaping of the American linguistic conscience from colonial times to World War I.

Arthur M. Schlesinger in Learning How to Behave (1946) traces the development of the etiquette book in America. He discerns three successive stages in the influence of etiquette--as "minor morals," law, and art. In the seventeenth century, particularly in New England, manners were equated with morals, and the earliest guides concerned matters of piety more than matters of social decorum.

Elder William Brewster of Plymouth colony owned a copy of the 1586 Pettie-Young translation of La Civil Conversatione, 1574, by Stefano Guazzo. This esteemed Italian Renaissance courtesy book defines civil conversation as "an honest commendable and vertuous kinde of living in the world." Conversation in its current, more narrow sense, i.e., speech, is discussed at length because "wee winne chiefly the friendship and good will of other, by the manner of our speech. . . ." (Lievsay, 1961, 35 & 20) Elder Brewster also had in his library a copy of the well-known conversation manual A Help to Memory and Discourse and other guides for the spiritual and practical improvement of the Puritans. The records of Boston booksellers show the demands for self-improvement literature, and, according to cultural historian Louis B. Wright, "Colonial Americans were determined that their conversation should not display a provincial rusticity if handbooks could prevent it." (Wright, 1957, 137)

Throughout the colonies people were publicly punished for breaches of linguistic etiquette--cursing, lying, swearing, name-calling. Women were admonished to suffer in silence, and the penalties for failing to heed this instruction were severe. The ducking stool was the punishment for common scolds, "troublesome, angry women, who by their brawling and wrangling amongst their neighbors, break the public peace, increase discord, and become a public nuisance to the neighborhood." (Jacob's Law Dictionary) In Plymouth slanderers were put into the stocks wearing signs announcing their offense; in Maine women were gagged for gossiping; in New York a cleft stick on an angry tongue brought silence. (Carson, 1966, 55)

In the colonial period, it was considered a moral responsibility toward the formation of a strong, virtuous character to read courtesy books. The authors were usually male and English, and often churchmen. The most influential in America was Richard Allestree, better known as the author of The Whole Duty of Man, 1658, described on the title page of the 1684 edition as "laid down in a plain and familiar way for the Use of All, but especially the Meanest Reader. Divided into Seventeen Chapters; One whereof being read every Lord's Day, the Whole may be read over Thrice in the Year. Necessary for all Families. With Private Devotions for several Occasions." It was widely read by both men and women, and George Washington and Thomas Jefferson both owned copies.

In another quasi-religious work, The Government of the Tongue, Allestree (1675) views the tongue as a slippery and tricky instrument, going without any warning and leading the undisciplined soul into lying, derision, flattery, boasting, and obscenity. He exhorts his readers:

> Let every man therefore be sure to begin at the right end of his work, to wash his own mouth clean, before he prescribe Gargarisms [gargles] to others. And to that purpose let him impartially reflect on all the undue liberties he has given his tongue, whether those which have bin here remarked, or those others which he may find in all Practical books, especially in (the most practical of all books) his own Conscience. (1675, 219)

Allestree also wrote separate books for the description of behavior particularly applicable to each sex, The Gentleman's Calling and The Ladies Calling. The Gentlemen's Calling, 1673 edition, discusses the vocation of a gentleman, the advantages of the position, and the

obligations which flow from his calling. The first duty of a gentleman is that

> . . . his Behavior will be affable and civil, not indolent and imperious. . .
> SECONDLY, his Words will be temperate and decent, the product of judgment, not of rage. . . .To this it is but proportionable, that they be also weighty and material. . . .; And both these qualifications are very essential parts of a Gentleman's dialect, the one opposed to light and foolish, the other to unseasonable discourse. . . . It should be the endeavor of those whom God hath endued with knowledge, to convey as much of benefit and instruction to others as they can: in order whereunto 'tis necessary they adapt their speech to the capacities of those they treat with, otherwise let them discourse never so elaborately, they will rather confound than edifie, and appear to affect more to boast, than communicate their knowledge. (1673A, 24-26)

The Ladies Calling, "which for over a century was accepted in England and America as final authority on the nature and duties of women," (Spruill, 1938, 213) is in two parts. Part 1 treats of the behavior associated with Modesty, Meekness, Compassion, Affability, and Piety; Part 2 explains the callings of Virgins, Wives, and Widows. Linguistic behavior is part of Modesty.

> Nor does she [Modesty] only refine the language, but she tunes it too, modulates the tone and accent, admits no unhandsome earnestness or loudness of Discourse. . . .A Womans tongue should indeed be like the imaginary Musick of the Spheres, sweet and charming, but not to be heard at distance. (1673B, 6-7)

Ladies are further warned to avoid those breaches of Modesty to which the sex is particularly susceptible--"meer chatting, prattling humour" and "the reveling of secrets." And nothing is more offensive than an oath:

> . . . 'Tis true indeed, an Oath sounds gratingly out of whatever mouth, but out of a woman it hath such an uncooth harshness, that there is no noise on this side Hell can be more amazingly odious. . . (1673B, 13)

Many other seventeenth-century English courtesy books found willing readers in the colonies. Among these were Henry Peacham's The Compleat Gentleman, 1662, and Richard Brathwait's The English Gentleman, (1633) and The English Gentlewoman, (1631). Brathwait's two volumes of 680 pages are dedicated to Thomas Viscount Wentworth and the Lady Arabella Wentworth as living examples of gentleman and gentlewoman and are printed with outlined margins in which are cited the sources of the advice in the text.

In the eighteenth century the book of parental advice became a favorite. Lord Halifax's The Lady's New Year's Gift, or Advice to a Daughter, 1668, went through fifteen editions between 1688 and 1765. (Spruill, 1938, 216) Dr. John Gregory's A Father's Legacy to His Daughters appeared in America in 1775, and, despite the Revolutionary War, over 20,000 copies were sold by 1780. (Mott, 1947, 304)

Children and young people were urged to take their manners seriously. Eleazar Moody's The School of Good Manners, which was

reprinted frequently during the eighteenth century, contains the following advice: "Never speak to thy parents without some title of respect, as, sir, madam, &c."; "Among superiors speak not till thou art spoken to"; "If thy superior be relating a story, say not, 'I have heard it before' . . . . If he tell it not right, snigger not." (Schlesinger, 1946, 4) At the age of fifteen George Washington, using an English version of a seventeenth century French manual, wrote out for himself 110 maxims of good conduct, entitled "Rules for Civility and Decent Behavior in Company and Conversation." (Cable, 1969, 42) Benjamin Franklin, concerned about his daughter Sally's education in his absence, wrote to his wife: "I hope she continues to love going to Church, and would have her read over and over again The Whole Duty of Man and The Ladies Library." (Spruill, 1938, 218)

Throughout the colonial period the standard of behavior presented in the courtesy books was ethical and English. The colonists sought to establish in America the privileges and refinements of the English gentility, accessible to anyone who would fear God and work hard to improve himself. In matters of the English language, the colonists welcomed eighteenth century prescriptivism and its dictionaries and grammar books, for these guides enabled them to speak and write with confidence among their betters. Even Benjamin Franklin accepted Hume's criticism of Franklin's use of new words and promised to correct his errors. (Boorstin, 1958, 278)

Daniel Boorstin, in The Colonial Experience, explains the Americans' willing acceptance of linguistic authority.

> Before guides to correct usage existed, a man learned his speech as he learned his manners and his place in society, from his father and mother. . . . Before mid-18th century a man did not consciously learn, and did not need to be taught, the 'proper' language of his social class, for he drank it in with his mother's milk. The very idea that there was a single 'proper' speech which any literate person could learn from a recipe book was subversive of old ways and the old caste. It is easy to see why this way of looking at language would suit the New World. (1958, 283)

The posthumous publication of Lord Chesterfield's Letters to His Son in 1774 brought the word etiquette and a different spirit to courtesy. Chesterfield instructed his son in all the minutiae of behavior that would enable the youth to favorably impress others. "In Chesterfield's view good breeding was founded not upon consideration for others, as the moralists taught, but upon consideration for self." (Schlesinger, 1946, 11) Selfish motivation aside, Chesterfield's work contained many excellent maxims of social deportment and was eagerly received in America in original form, in expurgated editions, and in a series of American adaptations. (Mott, 1947, 31)

The use of the word etiquette to mean "rules of proper behavior" derives from Chesterfield. In his letter of March 19, 1750, he writes, ". . . . pray get somebody to present you to him [the pope] before you leave Rome; and without hesitation kiss his slipper, or whatever else the etiquette of that Court requires." (Chesterfield, 1774, 313)

Chesterfield repeatedly lectures his son on the importance of his language and corrects by return letter any inelegancies or mistakes of

grammar or usage in his son's writing. During the summer of 1748, he waged a campaign against his son's slovenly enunciation:

> Your very bad enunciation runs so much in my head, and gives me such real concern, that it will be the subject of this, and I believe, of many more letters. . . .Good God! if this ungraceful and disagreeable manner of speaking had, either by your negligence or mine, become habitual to you, as in a couple of years more it would have been, what a figure would you have made in company, or in a public assembly?" (Chesterfield, 1774, 165)

In his October 2, 1749, letter Chesterfield is at work on his son's diction.

> I am very willing that you should make, but very unwilling that you should cut, a figure with them [diamond buckles] at the Jubilee; the cutting a figure being the very lowest vulgarism in the English language. . . .The word vast, and vastly, you will have found by my former letter that I had proscribed out of the diction of a gentleman. . . ." (Chesterfield, 1774, 260)

In the half century following political independence Americans continued to look to England for models of social propriety. Many of the older moralistic guides continued to be reprinted, but more and more etiquette came to mean a set of prescriptions to define proper behavior in social situations and was accorded the status of law. By the 1820's the manuals became devoted almost wholly to social decorum, and the new ones were written mainly by Americans. With rapid territorial expansion and the election of Andrew Jackson as president, persons of humble origin became more convinced that they could better themselves, and etiquette books became increasingly important. From 1830 to the Civil War more than 100 different guides appeared, not counting revisions and new editions. (Schlesinger, 1946, 18)

Ironically, it was during this period of increased attention to polite behavior that Americans were reproached for their lack of manners by the English woman Frances Trollope in Domestic Manners of the Americans (1832). Regarding conversation she complained:

> I have listened to much dull and heavy conversation in America, but rarely to any that I could strictly call silly (if I except the everywhere privileged class of very young ladies). They appear to me to have clear heads and active intellects; are more ignorant on subjects that are only of conventional value, than on such as are of intrinsic importance; but there is no charm, no grace in their conversation. I very seldom during my whole stay in the country heard a sentence elegantly turned, and correctly pronounced from the lips of an American. There is always something either in the expression or the accent that jars the feelings and shocks the taste. (1832, 56)

Mrs. Trollope became the subject of parody and caricature and made the nation more determined to acquire refined manners. In the two decades following Mrs. Trollope's condemnation, books like the following streamed from American presses in the East and made their way with the settlers to the frontiers of civilization: The Young Man's Own Book, 1832; A

Manual of Politeness for Both Sexes, 1837; The Laws of Etiquette, 1839; A Manual of Good Manners, 1844; Maberley's Art of Conversation, 1844; and Docine's Manners Maketh Man, 1852. (Wecter, 1970, 164)

One of the most popular manuals of this period was The Young Lady's Friend (1836), by the American lady Eliza Ware Farrar. In the chapter on conversation the young reader is cautioned particularly against exaggeration in conversation.

> A young lady, for example, told me she was 'pasionnately fond of embroidery.' She had used the word, in the sense of very, till she had lost all perception of its true meaning. (1836, 379)

Addressing herself to young ladies' anxieties about conversing with young men, Mrs. Farrar counsels:

> . . . it is much better to talk with them [young men], and with your female friends, of things than of people; of books, pictures, and the beauties and wonders of nature, than of Miss A_____'s spoilt complexion, or Mr. B____'s broken engagement, or the quarrel between C____ and D_____. If you are familiar with the works of great minds, and spend much time reading them, or if you love nature and scientific researches, you need not be told to avoid gossip, you will have no relish for it. (1832, 299)

A later guide, Emily Thornwell's The Lady's Guide To Perfect Gentility (1856), gives more specific advice:

> A lady should not say 'my husband,' except among intimates; in every other case she should address him by his name, calling him 'Mr.' It is equally proper, except on occasions of ceremony, and while she is quite young, to designate him by his Christian name. . . . . Do not use the terms 'genteel people'; 'this, that, or the other is very genteel.' Substitute for them, 'They are highly accomplished'; 'He is a gentlemanly man'; 'He has a gentlemanly appearance'; 'She has the manner of a gentlewoman.' . . . . Do not use such words as 'I guess,' 'I calculate,' 'I expect,' 'I reckon,' too often, and, as they are generally used, out of place.

Schlesinger points out that the etiquette books of this period were democratic and patriotic. No longer was the proper behavior of inferior to superior described, and readers were warned not to remind others of their humble origins nor to exaggerate or glorify their own lineage. Etiquette was for everyone, and by the eve of the Civil War, with the publication of the Dime Book of Practical Etiquette in 1859, nearly everyone could afford it. (1946, 21-26)

The period between the Civil War and World War I saw etiquette elevated to the status of art. Fabulously rich families whose fortunes were made by business acumen constituted a social élite whose conduct set the standards of etiquette. Simple and decent manners would no longer suffice. Manners had to show sophistication and elegance; etiquette was an art to be painstakingly cultivated. (Schlesinger, 1946, 27-35)

The illustrated Hill's Manual of Social and Business Forms (1873) gives a detailed picture of the daily concerns of Americans in the Victorian era, offering advice on such disparate topics as Jolly Christmas toasts and tasteful tombstone epitaphs. It includes sample

letters for various occasions and sections on "Gestures in Oratory and Elocution" for women and men, with illustrations and sample texts to accompany twelve moods. This remarkable how-to-do-it book was publicly endorsed by General Grant and the governor of Massachusetts.

One of the greatest challenges of the period was mastering the etiquette of the parlor, and all etiquette books discussed the length of the visit, proper attire, use of the calling card, topics of conversation, etc. The forbidden topics of polite conversation were called to mind by the five D's: dress, disease, domiciles, descendants, and domestics. (Lynes, 1963, 96) Slang, of course, was to be avoided, and the newly arrived were especially warned against using overly euphemistic and delicate expressions like "partake of liquid refreshment" for "have something to drink" or "reside" for "live." (Cable, 1969, 223)

The "best people" of the era adopted various continental customs and fashions and incorporated French phrases into their vocabulary. A lady could pick up a number of French words simply in reading her etiquette book. Constance Harrison's The Well-Bred Girl in Society (1898) uses French accent marks in débutant, régime, fête, séance, etc., and French phrases like robes en train, à fleur de tête, fin de siecle, en bloc, and chef d'oeuvres.

The lady's new cosmopolitanism was also to be reflected in the subject matter of her conversation. Correct Social Usage of 1906 suggests, "There are always articles of virtu, photographs of foreign subjects, paintings or statuary to be admired, and the conversation may gradually center on the Egyptian obelisk, or the Tower of London." (Schlesinger, 1946, 36)

The drastic social changes that came with World War I, the roaring twenties, the great depression, and World War II did not diminish the popularity of etiquette manuals, for nearly 150 appeared between 1918 and 1945. (Schlesinger, 1946, 51) In 1923 Emily Post's Etiquette: The Blue Book of Social Usage was the non-fiction best seller, and the twelfth revised edition is currently available at every bookstore. The name Emily Post has become synonymous with correct behavior.

At heart, the etiquette manual is a self-improvement book of the sort that Americans have trusted in since the early colonial era. Judging from the popularity of etiquette books such as those just surveyed, Americans have sought authority in social behavior, including conversation, and have willingly accepted the role prescribed for them there.

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THE DEFINITION AS A SPEECH ACT*

Joan Manes
University of Virginia, Charlottesville, Va.

Uriel Weinreich, in considering the problems of semantic description, suggests that defining is probably a "cultural universal" and that therefore it should be possible "to obtain tentative definitions from naive speakers of any language" (1967:42). Casagrande and Hale, in their article on Papago folk-definitions, discuss the fact that definitions are found in natural speech. They point out that "there will inevitably be occasions when the meanings of particular words must be explained to language learners, whether children or adults," and that despite the availability of dictionaries in our own culture "we too must often be practical lexicographers" (1966:165). My own fieldwork confirms their comments. We do frequently look up words in dictionaries, especially when there is some disagreement about the exact meaning; however, when someone asks for a definition of a word, we do not generally respond by looking it up or telling him to look it up (except, occasionally, for a parent speaking to a child). Furthermore, as discussed below, when we use a word with which we believe our hearer to be unfamiliar, we often give unsolicited definitions. Definitions, at least in American English, occur frequently and in a variety of situations. Defining can thus be seen as what Dell Hymes has termed a "speech act" (1972:56). The question which must then be addressed is: what are the rules for the use of this particular speech act?

If we examine definitions which occur in natural contexts, we find that definitions are actually the second part of a two-part speech event, the first part being a request for a definition. There are normally two participants involved, whom we may designate the requester and the definer. There are situations in which one or the other portion of the event (request or definition) may occur by itself; nevertheless, we still find two participants. In the case of unsolicited definitions, which are fairly common, the definer simply gives a definition without waiting for a request from his hearer, presumably because he believes the hearer to be unfamiliar with the term involved. Requests without definitions are infrequent, but there are situations in which the would-be definer may ignore or refuse a request. Both these occurrences are best analyzed as deviations from the norm of request followed by definition.

Since the typical definition is a response to request, it is important to examine these requests as part of our consideration of the definition as a speech act. The most common way of asking for a definition is to say, "What's a _____?" or "What's _____?" Of a total of 137 requested definitions for which the exact wording of the request was recorded, 56 were in response to a question of this sort. The only

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other questions that are at all common are "What's that?" (17 occurrences) and a repetition of the word with question intonation (12 occurrences). In four cases both appeared, as in "Tract? What's that?" From the variety of questions found, most of them occurring only once or twice, it appears that almost anything indicating lack of comprehension can result in a definition. Indeed, the request for a definition (or the utterance which elicits a definition) need not even be in the form of a question. All of the following elicited definitions in natural contexts:

A: ...nine dollars a rick.
B: Nine dollars a what?

What does warped mean?

But I don't even know what circulars are.

'Pall' isn't spelled that way.

A: We called them slam books and...
B: They're just sort of little poems in there?
A: The autograph books were poems, but the slam books were something different, where...

In the last two examples there is no overt request for a definition at all. Rather, the definition is given to correct a misunderstanding: in the first case, the speaker doesn't realize that 'pall' has a homonym 'pawl'; in the second, 'B' thinks slam books contain poems.

In what situations and from whom may one request a definition? I found (or elicited) definitions in all of the following situations: conversations among friends or acquaintances; word games; interviews; service encounters; child-adult (usually child-parent) conversations. There are undoubtedly many other situations in which definitions may be requested. In a classroom setting, for example, both students and teachers can request definitions. Nevertheless, we may assume that the situations listed above represent a fair sample of those in which requests for definitions occur, and examine them as such. It should be noted, before going on to such an examination, that there are situations in which one cannot request a definition, for example, during a church service or a large public lecture. (Of course, one might ask the person in the next seat what was meant, but presumably only if he or she were a friend, making the situation analogous to a friendly conversation in an unusual setting.) This is not to say that definitions could not occur in such formal situations, merely that they would be unsolicited definitions, given by the minister or speaker.

To return to those situations in which we do find requests for definitions, one thing which is common to all of them, with one exception noted below, is that in order to ask for a definition one must have some reason to believe that the person asked knows the meaning of the word. In conversations, interviews and word games, the reason is almost always that the person has just used the word. The other possibility is that the person's job or background makes it reasonable to assume that he or she is familiar with a particular term. Thus a

lawyer who was asked, during an interview, to explain the difference between a company and a corporation did so with no indication of surprise and no hesitation, although the request was entirely out of context. In a service encounter, the customer can ask not only about items which the clerk (or other service personnel) has just used, but about any term specific to the assumed area of job competence. Thus a waitress can be asked to explain a term on a menu, and a service station attendant will respond when asked, out of context, "What is my alternator?" (At least, when asked this by a woman; possibly the response would be different to a man making such an inquiry.) Presumably, in the case of a child asking an adult to define a word out of context the assumption is that adults know words with which the child is not familiar. As in the other situations discussed, a child may ask an adult to define a word which the adult has just used, and an adult may also ask a child for an explanation of a word which the child has just used and with which the adult is unfamiliar.

It is also possible, however, for an adult (usually a parent) to ask a child to define a word when there is no assumption that the child knows the meaning of the word. In fact, the purpose in such a case is to find out if the child understands the particular word, frequently but not necessarily one the child has just used:

Child: We're doing preterit in Spanish.
Parent: Good. What does it mean?
Child: Preterit tense? Past.

Parent: Do you know what graft means?
Child: Yeah, like graft paper.

In the second instance, the exchange was followed by a definition of graft given by the parent. This was possible because, when a parent (or any adult) asks a child to define a word with no assumption that the child knows the meaning of the word, there is in addition no assumption that the adult does not know the meaning. Quite the contrary, the adult is testing the child's knowledge and must, therefore, know the correct answer himself.

Although the majority of definitions are responses to requests of one sort or another, we do find quite a number of unsolicited definitions. Specifically, a survey of my data reveals 168 definitions given in response to requests, and 84 unsolicited definitions.[1] We have already noted that in some cases a definition is given in response not to a direct request but to a statement or question which indicates ignorance of the meaning of some term. In unsolicited definitions there is no overt indication whatever of lack of knowledge. However, the definer believes, for one reason or another, that some word or phrase he has just used is unfamiliar to his listener.

Definitions elicited during interviews in which the subject was asked to describe how to play some game include a very high proportion (over 75%) of unsolicited definitions. It is obvious that in the process of explaining how to play a game to someone who knows little or nothing about that game, one will be bound to use terms which are unfamiliar to the listener. (In fact, this is why this type of interview

was used.) In addition to requesting that the subject describe the game as if he were speaking to someone who knew nothing about it ("a foreigner"), the interviewer made it very clear that she herself did not know much about the game by asking for definitions or explanations of terms like 'dribble' or 'single'. When the speaker is thus made aware of his listener's ignorance, it is natural for him to give further definitions without being asked outright. It should be noted that for four of the six persons who described how to play some game, the first definition given was requested by the interviewer; of the two who gave definitions immediately without being asked, one had just listened to a friend of his being interviewed (and being asked to explain the meaning of various terms).

We have said that unsolicited definitions occur when the speaker (the definer) believes that his hearer does not know the meaning of a particular word or phrase which he has just used. If the speaker is unsure, he may ask the hearer whether he knows the meaning of the term in question. For example:

A: You can get everything around here. You just gotta go uptown to get it. THC. You know what THC is?
B: Uh-uh.
A: Well, you, you, you can shoot it up, you can take it, you can sprinkle it on grass. It gets you all messed up.
B: Hm, well what's it made out of?
A: I don't know.

In this case A suspects that B may not know what THC is, but is not sure enough to just go ahead and give the definition. He therefore asks first. There is a good reason for his doing so. Since an unsolicited definition is based on an assumption of the other person's ignorance, it may be considered somewhat insulting. This is apparent in the following dialogue:

A: ...they have to put bales and containers on pallets -- you know, little squares of wood (gestures to show shape and size) that you can grab onto,
B: I know what a pallet is![2]

Definitions occur, then, in response to three general situations: a definition has been specifically requested; a person has indicated ignorance of some term without actually asking for a definition, either by making a statement showing misunderstanding ("'Pall' isn't spelled that way") or by responding in the negative when asked if he or she knows the meaning of the term; the definer assumes that his hearer does not know the meaning. Clearly, in each case the basic function of the definition is informational: the definer is telling his hearer something he did not previously know. This is not, however, the only possible purpose to which definitions may be put. As noted above, parents may request definitions from children to test the child's knowledge. In such a case, the function of the definition is to show the parent that the child does (or perhaps does not) know the meaning of the term in question. We have already said that the assumption by

the definer of his hearer's ignorance (in the case of an unsolicited definition) may be insulting. In some cases the act of defining may itself be used as a deliberate insult, the definer pretending to assume that his hearer does not know the meaning of a simple word. Frequently, as in the following example, it is a joking insult:

> You gotta go front; that means forward.

Or the definition itself may be a joke:

> A stewardess is a waitress in high heels.

Clearly the informational content of this definition is practically non-existent.

The above are cases in which the definition itself is not informational, but serves some alternative purpose. However, even when the definition is informational in character, the act of giving the definition may have some additional function. The major such function is maintenance of the understanding necessary to conversation. When we speak to someone we assume that he knows the meanings of the words we are using. If that assumption is, or seems to be, invalid, a definition will be provided, not simply to tell the listener what the word means, but to tell him this so that we may continue the conversation. Thus if a speaker uses a word which is unfamiliar to his hearer, and the hearer requests a definition or explanation, the original speaker must respond with a definition before continuing. (There are certain exceptions to this rule, as discussed below, but they do not occur in normal conversation between adults.) That the purpose is to give enough information and not necessarily a complete definition, is shown in the following example, where the definer was interrupted as soon as he had given enough information for his listener to grasp his meaning. The definition itself is clearly incomplete.

A: Do you know what a container ship is?
B: No.
A: Well, you've seen these thirty, forty foot trailers. Some of them are just boxes on a (frame)[3]---
B: Oh now I know what you mean.

On the other hand, if the definer does not give enough information, his listener may inquire further, as "B" did when given a definition of THC.[4]

In some cases the definition is requested not because the word itself is unfamiliar to the requester, but because its use seems strange. The phrase "define your terms" is found in intellectual discussions of all sorts, but requests of this kind (although not using this particular phrase) occur in other situations and in reference to rather common terms as well. In an interview about growing up in Philadelphia,[5] we find the following interchange ("A" is the subject, "B" the interviewer):

A: Every block the neighborhood just a little bit tougher. So ---

B: What do you mean by tougher?
A: O.K. Two different meanings of tough. You can go into a tough neighborhood it's tough because thirty kids will jump you, well I consider this bad, not tough. Here I consider it, two men they meet on the street they don't argue they punch, that's tough, O.K.

Unsolicited definitions of this sort also occur:

> ...people who are successful, what I mean by that is, able to get themselves a job they are glad to have....

The speaker was talking to a friend who, like herself, is a graduate student. Her assumption was not that her friend was unfamiliar with the word 'successful'; rather, she wanted to make clear exactly how she was using the word, what type of success she was referring to. In situations like these the major purpose of defining is still the maintenance of necessary understanding.

There are cases in which a definition is informational, and the giving of the definition does maintain mutual comprehension, but in which an additional function is served. The definer may use the opportunity provided by his listener's lack of knowledge to assert his own superiority in a particular area. In the following example the definer, "A", is a man in his fifties with a fourth grade education.

A: ...and I worked as a sandhog -- you know what a sandhog is?
B: No, what is it?
A: You don't? Well, wait a minute now, you say you go to college and you d'know what a sandhog is?
B: No.
A: Well, let me tell you what a sandhog is -- uh, becau -- a sandhog is, you see bridges built?
B: Yeah.
A: You have to go down in the bottom of the ocean and, uh, 'til you find solid rock.
B: Right.
A: Under the ocean there's solid rock. And that's what they call your sandhog.

Finally, there is one situation in which the giving of information is the only function of the definition; this occurs when a definition is requested out of context, or at least outside of the context of a continuing conversation of any sort. When, for example, a customer in a filling station asks for and receives a definition of the term alternator from the attendant working the gas pump, the request - definition sequence constitutes the entire speech event. The definition serves no function other than a response to the request, and therefore its informational content is the only thing of importance.

Exactly the opposite is true for many definitions given in the context (if one can call it such) of word games. Here definitions are frequently offered as evidence that the particular set of letters is indeed an English word. Any actual information about the meaning of

the **word** is irrelevant, or at least very secondary, and the speaker can therefore give as much or as little information as he chooses.[6] In this situation we find both very complete definitions:

A: What's titre?
B: That's the balance of reagents in a chemical reaction.

and definitions which contain little or no information:

A: (question about ketone)
B: It's a chemical thingey.

Of course, the information given in any definition may be more or less complete, although we do not find definitions as vague as that of 'ketone' except in word games. The major factor involved is the situation in which the definition occurs, which allows the speaker to assume a certain amount of prior knowledge on the part of his listener. For instance, a waitress in a "donut" shop, when asked the difference between a "donut" and a cruller, said:

A donut is raised dough; a cruller is cake dough.

Since the question was asked in a "donut" shop, there was no necessity for the girl to explain exactly what "donuts" and crullers were. When a student, discussing the possibility of going to graduate school, said she had been offered a readership and was asked what it was, she said:

It's my tuition plus 600 dollars a year. I get to grade papers.

It was unnecessary to say that it was a type of scholarship or fellowship, or to go into details of these ways of paying for graduate school. The speaker correctly assumed that the fact that she was discussing graduate school, combined with the hearer's general knowledge, was enough to make the definition meaningful. The person giving a definition may make assumptions about the hearer's general knowledge not based on anything in the specific situation. The woman who gave the following definition of blind date to her 11-year-old daughter assumed that her daughter already knew the word date:

Blind dates is when you don't know -- someone, uh, introduces you to somebody, but you don't meet him until he walks in your front door.

The knowledge which the speaker assumes the hearer to have is not necessarily knowledge of the meaning of particular words. For example, in giving the following definition the mother assumed that her daughter remembered seeing her strain spaghetti in a colander.

A: What's a colander?
B: It's what you strain spaghetti with.

We also find complete definitions (i.e., ones which assume no prior

knowledge of any sort) in a variety of situations. In addition to the many words which are defined by synonyms there are definitions such as the following:

A: What is sachet?
B: It's a kind of scent that you put in your drawers to make your clothes smell good.

Here the only assumption made is that the hearer knows the meaning of the words used, an assumption we always make when speaking. Those situations in which this assumption cannot be made, or in which the assumption is incorrect, are exactly the situations in which we find definitions (unsolicited or requested). This, by the way, can happen even within a definition:

A: You're kinda spacey.
B: What's spacey?
A: Spacey is like pleasantly dingey.
B: What's dingey?
A: Dingey is like pleasantly nutsy in a complimentary way.

We have said that definitions are usually responses to a request, although unsolicited definitions also occur fairly frequently. A much less common occurrence is an unanswered request for a definition. There is a simple reason for this. Definitions are generally requested only when the person being asked for a definition has given the requester reason to believe that he does indeed know the meaning of the word, either by using it or because of his occupation or background. (The exception is when the requester is merely testing the other person's knowledge, most often, a parent testing his child's knowledge.) To refuse to give a definition therefore amounts to an admission that one does not have the knowledge which may reasonably be expected of one. This means that, except in certain specific types of situations described below, it is practically impossible to refuse a request for a definition in natural speech situations. Because of this, it is not uncommon for people to give inadequate or confused definitions of words whose full meaning they are not clear about, but which they apparently feel compelled to define:

A transistor is a little solid matter that doesn't throw off heat and therefore doesn't lose energy.

In one case the speaker apparently felt that the definition he had given was inadequate or incomplete and so kept adding to it, getting more and more confusing as he went on:

A: Loran.
B: What's that?
A: That's um, electronic means of, uh, telling where you're, are. It's, uh, radio waves that are sent out by, shore stations and, go out, cross the water...
B: Uh-huh.
A: And they cross, (they got 'em) crossing.

B: Oh.
A: Then you have a trans -- a receiver, on board ship, receives these waves and turns 'em in, to, uh, lights 'em up, you know, like a television set.
B: Oh, I see.
A: And you match the lines up.
B: Uh-huh, on a screen?
A: On a screen, yeah, and, uh, you get, uh, two lines crossing.
B: Mm-hmm.
A: And that's where you are, 'n', uh, chart has, um, lines on it.
B: U-huh.
A: And you just -- they're numbered.
B: Right.
A: And there's a number on the loran screen.
B: Uh-huh.
A: And you take the, it's in microseconds, the number of microseconds, that you get, you look on the chart and you find it.
B: Mm-hmm.
A: And you draw a line across that line, and then, microseconds, you get going this way, would, uh, you cross that and where they cross is where you are on the chart.
B: Oh, I see.

There are, however, cases where a person can refuse to give a definition of a word which he or she may reasonably be assumed to know. Doctors sometimes do so when asked by a patient to define a medical or technical term which they have just used. In one case, a doctor refused to define 'synergistic', saying that it was a medical term which there was no need for his patient to know. Parents avoid, though not necessarily by outright refusal, defining "dirty" words to their children. In fact, adults can simply ignore requests for definitions from children, although they cannot, at least in social situations, ignore such requests from other adults. I have one definition, of cabin cruiser, which was given only after three or four requests on the part of the child. Teachers, too, can refuse definitions by indicating that the request does not pertain to the topic under discussion, or is otherwise inappropriate. It would seem, then, that in situations where one person has authority of some sort over another person, the person in authority can use that authority to refuse a definition requested by the other person.

To sum up, definitions occur when one person presumably knows the meaning of a word or phrase, and the other person presumably does not. In this situation the former may simply give a definition, unsolicited, or the latter may request one, in one of a number of ways. Such a request cannot, in general, be denied. The definition itself is explanatory in purpose, although the completeness of the explanation depends on a variety of circumstances, including the context in which the definition is given, the assumptions which the speaker is able to make about the general knowledge of the hearer and how much information the hearer feels he needs. There are of course a number of variations, as discussed: definitions may be used as jokes, or to show one's

superiority; definitions can be requested by someone who already knows the meaning of the word, in order to find out whether the other person knows the word; in certain situations a person can use his or her authority to refuse a requested definition. Nevertheless, this description represents the basic setting and function of the majority of natural definitions.

FOOTNOTES

1. The total is somewhat less than the total number of definitions collected for three reasons: for a number of definitions, I do not have a record of whether or not they were responses to questions; in some cases one question elicited more than one definition, either because more than one word was asked about or because more than one person answered; finally, there are some definitions which fall into a third category, where the speaker first asks the hearer if he or she knows the word and then gives the definition, as discussed below.

2. The bracket indicates that both people were speaking at the same time. I found no other examples of people overtly expressing irritation at being given a definition, but this may well be because most of the definitions were collected by myself. Since I was trying to find or elicit definitions, I certainly did not want anyone to feel that he might be insulting me by giving one. The fact that the investigator's interest in a certain event may itself affect that event is, of course, a major problem in linguistic (and no doubt other) fieldwork.

3. Words in parentheses are those about which there is some uncertainty.

4. I have only this one example of a listener inquiring further into the meaning of a defined term, and very few examples of interrupted definitions. Here again, my own interest may be involved. Since I was collecting definitions I did not interrupt when they were given. I did not ask further questions once a definition was completed, partly because I did not want to call attention to my interest; if I had done so, it could not in any case be considered valid, natural data. (Of course, interruptions, further questions and expressions of irritation [see footnote 2] may simply be extremely infrequent. The problem is that this cannot be determined from my data because my interest <u>may</u> be interfering.)

5. I was not the interviewer, and the person who did the interview was interested in other linguistic data, not in definitions. For this reason his request for a definition may be considered natural.

6. In fact, the response "I don't know, but I'm sure it's a word" is not only acceptable but quite common.

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THE PLACE OF CODE SWITCHING IN LINGUISTIC CONTACTS

Douglas Shaffer

SUNY College Cortland

Linguists have long recognized that languages in contact can affect each other. Linguists have also long recognized that language has a hierarchical internal structure. It is these two cardinal principles that are reflected in varying degrees in much of the terminology for studies in bilingualism. Lexical borrowings have been the most visible reflection of contact and so have historically attracted the greatest amount of study. Foreign accents are another readily apparent result of contact, and advances in phonology over a century have combined to yield research in phonological borrowings as well. Uriel Weinreich (1953) and Michael Clyne (1967) have developed typologies for interlingual impact that are much more comprehensive than previous attempts. (1) Interlingual impact, termed "interference" by Weinreich and "transference" by Clyne, was then subdivided in both schemes according to the structural level at which it occurred.

Einar Haugen (1956) adopted a somewhat different approach. Basically his typology for interlingual impact reflects different degrees of bilingual code distinctiveness. Instead of speaking simply of interlingual impact, he said that languages in contact diffuse in three mostly discrete stages: code switching, interference, and integration. By this scheme switching constitutes the alternate use of distinct codes; interference constitutes "overlapping"; and integration constitutes interlingual impact only in a historical sense. In other words, integration (known otherwise as borrowing) amounts to transference that is generally accepted as an integral part now of a given language, a language that over time has changed through contact.

According to Haugen's scheme interference and integration have in common a certain degree of leveling in the structural distinctiveness of both codes. By contrast, code switching involves the stringing together of "unadapted" words and phrases. (Haugen 1956:40; Haugen 1973: 528) On the other hand, interference is contrary to contemporary norms, whereas integration is in harmony with current norms.

Until the 1950s linguists had considered (and defined) bilinguals as having total native-like command of two languages. In such an ideal situation interlingual impact would not exist. However, as students of bilingualism have observed, bilingualism is seldom if ever balanced, and interlingual impact occurs in varying degrees. Several

extralinguistic factors affect the kind and degree of impact. Late acquisition of a second language often results in imperfect acquisition. Group bilingualism, especially where both languages are actively used over decades or even centuries, contributes greatly to both switching and borrowing.

People in contact bring languages into contact. Thus, Schuchardt and Loewe in the last century and Weinreich, Mackey, and Haugen in this century have all regarded the bilingual individual as the locus of interlingual impact. Those of one ethno-linguistic background are bilingual and communicate in a second language with members of the other ethnic group, itself monolingual except for a few individuals. Universal or even majority bilingualism never seems to occur within any communications network. Most bilingualism, in fact, is unstable and constitutes a transition from monolingualism in one language to monolingualism in another language across one or several generations. Stable bilingualism involves tight compartmentalization of ethnic contacts and thereby severe limitations upon language contacts and domains of use. Such compartmentalization reflects either geographic distance or social distance, as with caste systems. It would seem that stable bilingualism results from late acquisition of a second language, as each subsequent generation of young adults moves into contact with another language group, often for occupational reasons.

Within a close-knit or sizeable bilingual group the languages of the bilinguals affect each other, especially where bilingualism continues for several generations. (2) The bilingual group's ancestral language feels greater encroachment from the second language than does the second from the ancestral language, even if only code switching and lexical borrowing -- the minimal effects of contact -- are involved. Two basic varieties of the group's second language emerge. One is used only with other bilinguals and incorporates a certain amount of that group's first language. The other variety is used only with monolingual speakers (from the other ethnic group). It is closer to the dialects used by monolinguals themselves than is the variety used only among bilinguals; in many cases it is identical with the monolingual variety. For a bilingual group then three rather than two modes of speaking may develop over time (Weinreich 1953:81; Hasselmo 1970:202). Thus, transference and integration proceed mostly in one direction as the ancestral tongue changes in the absence of contact with ancestral-language monolinguals and increasingly in contact with monolingual speakers of another language.

Over time bilingualism results in a partial merger of originally distinct codes and concomitantly the emergence of new norms, norms which may even include a certain amount of switching. Nonetheless, no two languages ever mix together in equal proportions. Not only is the direction of change mostly one way, as already mentioned, but resistance to equal mergers is provided by the internal structures of both codes. Even pidgins and creoles, the closest things to a code merger, are never averages of the base codes. It would be more accurate to say that languages in contact are increasingly colored by each other.

Code switching seldom occurs in writing, though borrowing does. Switching is confined almost exclusively to informal speech, probably because it is often delivered more rapidly with less concern for suitable phrasing and word choice such as would be allowed by greater time in preparing a written text or as might be required by speaking in a formal situation, where one may speak slower as he weighs his words for effect. Indeed, switching follows the line of least resistance.

While accepting Haugen's term and concept of switching as the alternate use of different languages, Richard Diebold (1963:56) adds an important qualification: it is code alternation within the same discourse. All switching ultimately stems from individual histories and domains where one has learned to use each language. Every researcher has recognized social, or contextual, causes for switching. Bilinguals themselves are aware of situational appropriateness as one cause for using one language or the other. Termed "external conditioning" by Clyne, situational appropriateness is dependent upon such factors as the person addressed, the setting, and the topic discussed.

Yet Clyne observed that not all switching is dictated by situational appropriateness. Some switches are caused by the selection of particular lexical items. These switches, he said, are "internally conditioned", or "triggered". Clyne found further that triggering is either anticipatory or consequential. He then specified some kinds of words that can act as trigger words: cognates, proper nouns, loanwords. While today some of his trigger units might be deemed to have been too narrowly defined, it was Clyne's general concept of triggering -- the concept that one word can dictate the language of surrounding units -- that was so significant to a better understanding of switching.

In summarizing the thrust and direction of code switching studies of the 1960s, Haugen was most influenced by Clyne's concept of triggering when he pointed out that such research suggested the abstract formation of sentences, perhaps even before the speaker was certain which language the utterance would come out in (1973:530).

As Haugen used the term, interference would include all instances of overlapping. Such overlapping would necessarily include native carryover into an imperfectly learned second language, as well as both momentary and gradual convergence, as two languages overlap either across levels or -- as in the pronunciation of many switches in close proximity -- sequentially at the same level. While this author has essentially accepted Haugen's three-stage diffusion, a somewhat different definition of interference will be suggested here. Interference will be confined to imperfect second language acquisition. Thus, interference will be recognized as restricting the native-like distinctiveness of the two codes available to a bilingual. (Such imperfect learning from the outset of second language acquisition generally results from late acquisition in the life of an individual.) By this definition of interference, switching then involves all interlingual impact where a bilingual maintains separate codes more nearly intact according to monolingual community norms. Of course, switching tends in the

same direction as interference, although switch-induced integration is much more gradual. In this paper interference is considered to be instant transference through imperfect acquisition. Integration is transference nativized and accepted. Switching is held to be the broadest area of interlingual impact in between: switching is transference in midstream. Further implications of these adjustments in Haugen's definitions will now be explored.

In determining whether adaptation had occurred or not (i.e. whether integration or switching was involved), Haugen (1956:39-41, 50) had said that phonology and morphology were the determining factors. More recently, it has been suggested that adaptation is more accurately measured by syntactic considerations (Shaffer 1975).

The case for syntactic primacy in distinguishing switching from integration rests upon several factors:

First, switch boundaries correspond mostly to phrase structure boundaries. (3)

Second, a review of the data in print embodying code switching shows that the language of entire phrases mostly agrees with and thus would seem to be dictated by the language of the head word (Shaffer 1975). Further support for this view is provided by some of Clyne's data in which a bilingual informant gropes for the head word -- usually a content word like a noun or a verb --, fails to think of it in the language of the preceding span, and restates the entire phrase in the alternate language in which he more readily recalls the head word. For instance:

1. und ein -- a lighter.

2. Wir nehmen unsere Bücher für vier -- for four periods.

Bilingual informants themselves sometimes explain a switch by saying they could not think of the desired word in the same language.

Third, as noted by Diebold (1963) and Hasselmo among others, the phonology of switches may spill over across lexemes, especially where the switches are in close proximity rather than having been separated by a pause. Thus, some instances which researchers still wanted to classify as switches exhibited some measure of overlap. Phonology was not a totally consistent guide to identifying switches.

Instead, it has been argued that the language of elements surrounding a head word, including proper concord and agreement as marked by structure words and affixes within the phrase, is a more accurate indicator of switching as opposed to borrowing. Switching would therefore seem to involve entire phrases rather than single words. It is not without good reason that Clyne (1967:19) referred to switching as "multiple transference".

The code of the head word selects the code of the whole phrase.

Where situational appropriateness is not overriding, the head word is the one recalled faster from among bilingual doublets. Thus, lexical recall speed determines the language of whole phrases as well as of the head word. Discourse, especially within any sentence, as a rule continues in the same language right up to a phrase whose head word causes a switch. Lexical recall speed, as well as all other aspects of proficiency, is dependent upon ongoing frequency of encounter and use (4) -- a factor which likewise explains shifting competence among bilinguals, some of whom may even completely forget their first language and undergo a total language shift during a lifetime. Proficiency must be defined broadly enough to include not only the ability to generate grammatical utterances but also the availability of vocabulary for any given domain and concomitantly the speed of lexical recall.

Yet in everybody's data morphologically and syntactically unadapted words do appear. Donald Lance (1969), for instance, turned up examples like these:

1. ... se les cayera el skin -- el, tú sabes, cuerito.

2. ... en la fat -- en la grease -- tú sabes, en la manteca ...

The availability of bilingual doublets to the speakers, obvious in these examples, led Lance to interpret these as instances of switching. The American pronunciation of "skin", "fat", and "grease" also favored such an interpretation. Yet if, as has been argued, switching involves entire phrases rather than single words -- phrases replete with a vertical (hierarchical) as well as a horizontal (linear) dimension --, how can such examples as these be explained?

Lance, who at the time was unaware of the studies by Clyne and Hasselmo, wrote that "there are perhaps no syntactic restrictions on where the switching can occur" and cited as the reason a long list of syntactic groups within which and between which switches occurred in his data. Thus, Lance found what he interpreted as switching between an article and a noun or after a preposition or a conjunction (1969:76). Lance's not having recognized syntactic restrictions on switching in his data derived from his notion of what constituted switching as opposed to borrowing. In fact, when juxtaposed with Hasselmo's analyses and those on borrowing by Weinreich (1953, 1966) and Mackey (1970), Lance's data have important implications for the distinction between switching and borrowing, as well as for the very process of integration.

As Weinreich (1953:54) has shown, loans are introduced by bilinguals, and many a lexical borrowing coexists for a time with a native equivalent before either dropping out of use or encroaching upon the native equivalent's semantic range and occurrence. Mackey (1970) shows much the same, demonstrating that there are degrees of integration for any lexical item borrowed.

In fact, the process of borrowing involves for a while coexisting systems and items drawn from several separate codes (Weinreich 1966).

Coexisting systems have been documented for bilinguals and account for those areas where borrowings can occur yet retain the original phonology and/or morphology, even though a certain degree of modification of borrowings in conformity with the integrating language is far more common. The thrust of recent research has been that bilinguals are carriers of linguistic change and that in certain circumstances change spreads from them throughout monolingual segments of the population.

Thus, there appears to be a continuum between switching and borrowing. (5) It also seems that some items in switches are gradually borrowed. There would then be two roads to lexical borrowing: instantaneous borrowing (as in the case of sputnik, borrowed from Russian overnight), and gradual integration of items earlier used in frequent switches. Lending further support to this interpretation are data from Mackey, Hasselmo, and Clyne which reveal that numerous lexical items have assumed a neutral status and can be used as an integral part of either code.

Code switching affords a glimpse into the process of sentence formation and into the neural organization of language, since switching involves the generation of sentences from elements and systems of two largely distinct codes and since -- despite gradual and ever increasing impact -- the individual linguistic units can mostly be unambiguously assigned to one code or the other. Indeed, support for the psychological reality of phrase structure provided by recent code switching studies is seconded by some recent monolingual studies. (6) Victoria Fromkin's research into monolingual slips of the tongue, for example, suggests the psychological reality of IC models.

All studies in switching thus far, including this author's proposal early this year for syntactic primacy, have tacitly accepted Haugen's implied assumption that switching involves strings that are unadapted all the way to the level of lexicon. In other words, until now all switching has been assumed to involve either primarily (Haugen 1956) or minimally (Shaffer 1975) unadapted morpheme shapes; all switching has been assumed to be lexical switching.

Yet the question should now be raised as to whether there may not be such a thing as higher order switching. There are several indicators that prelexical switching does in fact occur.

First, if interference is relegated to imperfect non-pathological second language acquisition, then all transference involving intact codes must be switching. If, as has been suggested, lexical borrowing is mostly effected by code switching embodying lexical transfers (i.e. if lexical borrowing is shown to have come about through lexical switching), why should syntactic borrowing be assumed to have come about through lexical switching? It is simpler to assume that integration at a given level has been effected through switching at the same level.

Second, why should it be assumed that higher order transfers require lower order transfers in the process? Lexical switching indeed

suggests prior formation at higher levels in utterance generation. Haugen (1973:520-1) has already made the point that not only use but mere knowledge of two languages is sufficient to result in transference.

Third, loan translations -- including the semantic transfer of idioms -- likewise indicate prelexical switching. It should come as no surprise, therefore, that, as Weinreich (1953:109) and Haugen (personal communication) have maintained, loan translations are much more common in stable bilingualism -- the kind of bilingualism that most promotes (lexical) switching -- than in transitional bilingualism.

Clyne (1972:9, 33) documents what he calls "lexicosyntactic transference", the transfer of cooccurrence restrictions across levels (syntactic/lexical). These instances concern use of an English subordinate conjunction after a stretch of German and followed by German with the word order compatible with the language of identity of the subordinate conjunction (English). For example:

1. ... if der Vater hat keine Farm.

2. ... because der Boden ist zu -- steiniger Boden.

3. ... since mein Vater ist tot.

A tentative interpretation is that the subordinate conjunction is now integrated into the German at the lexical level (i.e. has neutral identity, since the head word and other lexical items in the IC are German) and that there is switching at the syntactic level. (Another alternative is that not only lexical integration but also syntactic integration rather than syntactic switching is involved here.)

One of the intriguing aspects of these examples is that in the other published data, no subordinate conjunctions assumed a neutral (or bilingual) status. Whereas content words, discourse markers (Haselmo 1970:185, 206-8; Clyne 1967:75), and even coordinate conjunctions were sometimes borrowed, subordinate conjunctions were not, presumably because embedding is a more close-knit construction than conjoining.

If prelexical switching is recognized, what causes it? Is it external and internal just as in the case of lexical switching? In lexical switching there appear to be no structural reasons for choice among bilingual doublets except for cognates (Weinreich 1953:48-50; Haugen 1953:380) and perhaps shortness of terms (Clyne 1967:79). Frequency and recency of encounter and use plus situational appropriateness seem to account for most lexical switching. Yet since the lexicon is less structured than other levels of language, it may turn out that structural considerations are far more important in prelexical switching -- either promoting or restricting such switching and presumably thereby affecting integration at the same level. (7)

Not only must the general causes of switching be better understood, but reasons for selection of a particular level for a switch should be investigated. Clyne (1972:41) suggests one set of conditions that can result in a syntactic transfer. When (for whatever reason) a speaker may have formulated a sentence in a language other than that of the conversation, he may relexify to keep the final output in the language of discourse. While syntax is clearly an important key to language identification, in many instances languages do not differ so drastically in their word order. Rather, most languages differ far more sharply in their morpheme shapes. Thus, laymen probably identify a language essentially according to morpheme shapes. Weinreich (1958) even argued along the same lines for classifying pidgins and creoles as relatives of their base languages. This fact of language identification combined with situational appropriateness may account for many instances of syntactic switching. Clyne also suggests that syntactic economy may favor lexical transfers (switches), as in the case of separable verbs in German occasionally being replaced by simple English verbs (1967:80-2).

In one very important respect Haugen's notion that switching involves lack of adaptation (1973:528) is retained here, inasmuch as levels above the switch are apparently all in the same language and those below are also probably all in the other language. Hasselmo (1972) presented evidence that suggests this hypothesis, although further research is needed to determine for sure whether vertical (hierarchical) re-switching is possible.

Previously transference and switching were not equated. Switching was thought to entail code alternation at the lexical level and thus to be a very restricted (and little researched) subtype of transference. In the present paper it has been argued that all transference involving two intact codes available to a speaker is switching. Furthermore, drawing upon Hasselmo (1972) and studies in sentence generation (8), it follows that since code switching seems to constitute ordered selection, all transference in midstream would seem to be ordered. Further, if all switching is ordered, this also probably indicates that all utterance generation is ordered, even that by monolinguals.

While phonological and lexical transference and borrowing are more easily studied, higher order switching should be explored as one means of fathoming the very processes of sentence generation and of change.

Notes

1. Cf. Clyne 1967:17-21.

2. Cf. Gumperz 1967:50, 55.

3. Hasselmo 1970:181-2; Clyne's data (personal communication)

also support this. A survey of published bilingual studies involving English as one language and German, Yiddish, Norwegian, Swedish, and Spanish as the other substantiates this as well (Shaffer 1975).

4. Cf. Belyayev 1964:52-7; Ervin-Tripp 1967:79.

5. Cf. Clyne 1967:18; Clyne 1972:10; Mackey 1970.

6. Leont'ev 1969:212-6, and the studies cited there.

7. Cognates, lexical items that are phonetically and semantically similar, have a high attraction for further convergence. Weinreich (1953:39-40) suggests that structure markers -- bound morphs and function words -- may be readily transferred when, similar to the conditions for cognate convergence (cf. Weinreich 1958:377), they are phonetically and functionally similar to existing native markers.

8. Leont'ev 1969; Clyne 1972:39-41.

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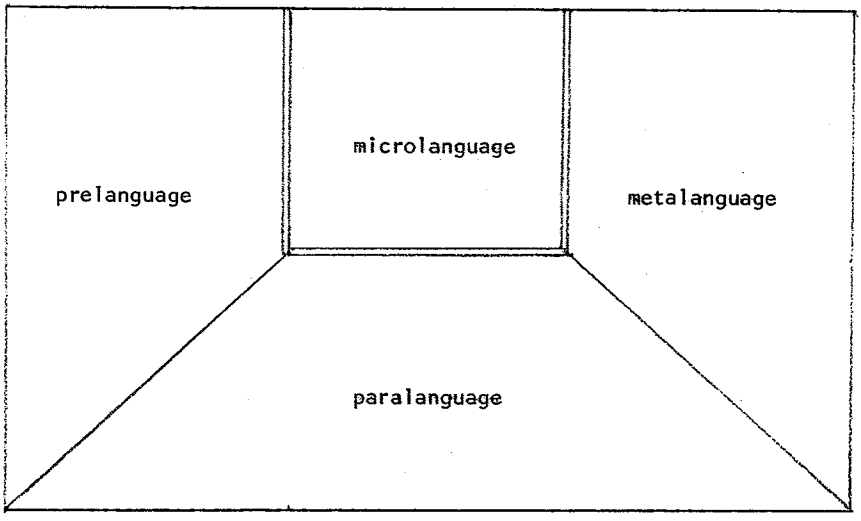
ALLOLINGUISTICS: Exploring the Peripheries of Speech

Roger W. Wescott
Drew University

In his article "Language" in the 1955 edition of the Encyclopaedia Britannica, George L. Trager distinguished language in the broad sense, which he called "macrolanguage," from language in the narrow sense, which he called "microlanguage." In addition to microlanguage, Trager's macrolanguage subsumed (1) prelanguage, such as pongid vocalization and infantile babbling, (2) paralanguage, such as non-grammatical interjection and speech-deformation, and (3) metalanguage, such as poetry and song.

Although Trager himself did not diagram these relationships, they can fairly be schematized, I think, as follows:

MACROLANGUAGE



Trager did not suggest any term for the three non-microlinguistic subdomains of the macrolinguistic domain when considered in collective opposition to the microlinguistic subdomain. But, since these three do have more in common with one another than any one of them has in common with microlanguage, I think it useful to coin an umbrella term for them. The unitary neologism that I propose is "allolanguage"; and I define it as language that is alienated from conventionally structured speech. The typological

contrasts between alienated and conventional language are perhaps best illustrated by means of a table of antitheses, as follows:

MICROLANGUAGE	ALLOLANGUAGE
gestureless	co-gestural
digital	analogic
conceptual	perceptual
symbolic	iconic
grammatical	grammarless
businesslike	playful
standardized	privatized
denotative	connotative
specific	polysemic

Needless to say, most of these antitheses should be interpreted in a gradient rather than a discrete manner, since the boundary between microlanguage and allolanguage is both blurred and fluctuating.

Moreover, some of the oppositions in the preceding table may require explanation, illustration, or amplification. Allolanguage is co-gestural, for example, in the sense that it is far more likely than microlanguage to elicit strong kinesic accompaniment from the speaker. Thus, the English phrase "went kerflop" is likely to be used simultaneously with a plunging motion of the hand, while the synonymous verb "fell" is not.

Allolanguage is analogic (in terms of information theory) in that differences between similar allolinguistic forms are likely to be of the more-or-less kind than of the all-or-none kind. As an adjective, for example, hip is merely a more up-to-date and hence forceful version of hep; whereas the same vowel contrast in a microlinguistic pairing like sit vs. set (in standard usage) is discrete, in that the second form is the causative alternant of the first.

Allolanguage is perceptual, rather than conceptual, in the sense that it is replete with echoics like the verbs clink, cackle, and croak, which not only echo themselves by means of consonant repetition but also echo non-speech sounds in the environment.

This perceptuality is iconic to the extent that speech-sounds exhibit a more than conventionalized resemblance to extra-linguistic realities. The small oral cavity used in producing such modifiers as

itty-bitty and teensy-weensy, both meaning "very small," is a case in point.

Allolanguage is grammarless in two senses--first, that many of its forms resist analysis and, second, that its structural "paradigms" are often absent from microlanguage. English examples of the first of these situations are the slang term jillion, "an indefinitely vast number," and the second half of the compound palsy-walsy, "excessively friendly"; and of the second, the word-pairs smack ~ smash and crack ~ crash, in which the forms ending in stops seem to have an instantaneous sense, whereas those ending in fricatives seem to have--comparatively, at least--a durative or resultative force.

Allolanguage is playful, in that it appears to involve sportive experimentation with speech-forms more for expressive than for informative purposes. An English example is the reduplicative form hippity-hop, which involves not only the kind of apophonic alternation seen in the masculine-feminine pairing fox ~ vixen but also the use of the mock-Latin suffix found, in its microlinguistic form, in learned words like deity and legality.

Allolanguage is privatized insofar as it lacks the conventional standardization found in most types of microlanguage. Privatization is seen in any kind of speech that is restricted to subgroups within the larger speech-community. It is characteristic of ethnic argot, occupational lingo, and the "little language" of lovers. The kind of words in which privatization is most readily observed is proper names, which, because they are regarded as the property of families or individuals, are accorded a variability of pronunciation and spelling not accepted in common nouns. This variability is even more marked among nicknames, which, in addition to polymorphy, exhibit multiple assignability to non-hypocoristic forms. Thus the name Mary, for example, has hypocoristic variants Minnie, Mamie, and Polly. And, in one family I know, the nickname used is Meenie. Minnie, moreover, may also be used as a nickname for Minna, Minerva, or Wilhelmina.

Allolanguage is highly connotative in the sense that it evokes impressions which, though vague, may be strong. A group of such allolinguistic forms consist of a stressless prefix beginning with a labial or velar stop followed by a stressed base with the nucleus /uw/. Examples are: patootie, "pretty girl"; bazooka, "rocket gun"; kafooster, "useless talk"; and gazoozle, "to cheat." All four terms are more emotive than intellective.

Finally, allolinguistic forms tend to have more meanings each than do microlinguistic forms. An example of such a polysemic lexeme is the noun lollop, which may be glossed as: (1) lounging or idling; (2) a loafer; (3) a person, especially an adult male; (4) bobbing up and down; (5) leaping clumsily about; (6) a blow or the act of striking; (7) a blob or portion.

Generally speaking, the thrust of microlanguage is toward decontextualization,[1] deaffectualization,[2] and disambiguation.[3] That is, microlanguage does not depend nearly as much as allolanguage does on context, where context is understood to mean situation, communicative body movement, and voice quality. Microlanguage, similarly, is oriented primarily toward quantifiable factual information rather than toward subjective personal emotion. And it maximizes the precise and specific as against the vague and unspecified. Thus, microlanguage loses little when mathematicized, less when written, and virtually nothing when telephoned. Allolanguage, by contrast, loses virtually all of its impact when mathematicized, most of it when written, and much when telephoned. To assure ourselves of this divergence, we need only try to imagine converting the following Walt Disney song-text into the logical notation of Whitehead and Russell's Principia Mathematica:

> Zippity-doo-dah, zippity-yay:
> My-oh-my what a wonderful day!

Synchronically, allolanguage is refractory to grammatical transformations of the Chomskyite Generative type[4] and, diachronically, to glottochronologizing of Swadesh's lexicostatistic type.[5] Its resistance to Transformational rewriting is due in part to its phonosemicity[6]--that is, to its tendency to move directly from sound to meaning without grammatical intermediation--and in part to the difficulty of interpreting refrains like the above couplet in terms of "kernel" sentences.

Allolinguistic resistance to glottochronological treatment is due to the fact that allolinguistic vocabulary does not seem to undergo the slow but steady replacement characteristic of microlinguistic vocabulary. Instead, the allolinguistic lexicon tends either toward evanescence or toward multimillennial persistence. The evanescent lexicon usually consists of slang and similar in-group argots, while the persistent lexicon consists of echoics and other iconic vocables. (Echoics, interestingly, tend not only to resist lexical replacement but also to resist sound-changes of the type covered by such formulae as Grimm's Law. Thus, the reconstructed Proto-Indo-European forms *pap-, "teat," *tut-, "to hoot," and "kuku-, "cuckoo," appear in English as pap, toot, and cuckoo, rather than, as comparative philologists would normally predict, *faf, *thuth, and *hough.)

The "grammarlessness" posited by Gerard Diffloth[7] as characteristic of allolanguage might better be referred to, in at least some circumstances, as "counter-grammar," since certain types of allolanguage have paradigms and other systematic structural features which differ sharply from the grammar of the corresponding microlanguage. In many languages, palimphony (sound-repetition) and apophony (sound-alternation) typify allolanguage, as they do in English. In English, moreover, internal affixation--which is part of the microlanguage in such language groups as Nilotic and Austronesian --is a marked feature of allolanguage.

Palimphony in English is of two types and several subtypes and infratypes. The two types are: endosyllabic, occurring within syllables; and cross-syllabic, occurring between adjacent syllables. Endosyllabic palimphony is generally called palindromy, palindromes being syllables (and, in most cases, also morphemes) which exhibit the same phonemic sequence when taken from back to front as from front to back.[8] English palindromes are of four kinds: progressive additive, in which a zero coda is suppleted by the syllabic onset; progressive replacive, in which a consonantal coda is replaced by the syllabic onset; regressive additive, in which a zero onset is suppleted by the syllabic coda; and regressive replacive, in which a consonantal onset is replaced by a consonantal coda. Examples of these four kinds of palindromes follow.

progressive additive: mom ~ ma
progressive replacive: peep ~ peek[9]
regressive additive: Nan ~ Ann
regressive replacive: Bob ~ Rob

Cross-syllabic palimphones are of two sub-types: simple and compound. Simple cross-syllabic palimphony, in turn, is of three kinds: alliteration, in which the syllabic onset is repeated; assonance, in which the syllabic nucleus or peak is repeated;[10] and reliteration, in which the syllabic coda is repeated. English examples follow.

alliteration: hee-haw
assonance: humbug
reliteration: kickback

Compound cross-syllabic palimphony is of four kinds: preliteration, in which both onset and nucleus are repeated; circumsonance, in which both onset and coda are repeated; rhyme, in which both nucleus and coda are repeated; and reduplication, in which all three are repeated. English examples follow.

preliteration (alliteration plus assonance): sad sack
circumsonance (alliteration plus reliteration): sing-song
rhyme (assonance plus reliteration): claptrap
reduplication (alliteration plus assonance plus reliteration): bye-bye

Some palimphonic sequences combine the preceding types of sound-repetition in complex ways. Endosyllabic and cross-syllabic palimphony co-occur, for example, in the compounds boob-tube (exhibiting palindromy and rhyme) and lollipop (exhibiting double palindromy and assonance).

No palimphones, however, equal what I call "word chains" in their systematic use of interdigitating sound-repetitions.[11] Word chains consist of three headwords, with optional intervening connectives, so arranged that, while the first and third words have

no systematic phonic link with one another, both show such links with the second word. The commoner type of word-chain concatenates rhyme with alliteration, as in the case of the Mother Goose refrain hickory, dickory, dock or the stereotyped phrase healthy, wealthy, and wise. The less common type of word-chain inverts canonical order and concatenates alliteration with rhyme, as in the fictional name Ralph Roister Doister or the literary slur Big Bow-Wow.

Apophony is phoneme-alternation of a systematic rather than a sporadic kind. The most familiar English examples are of course grammatical vowel gradations such as occur in verb paradigms like drink, drank, drunk. Their allolinguistic equivalents are sequences like clink, clank, clunk, in which the three syllabic peaks are iconically expressive, respectively, of a high-pitched sound (such as that of cocktail glasses being tapped), a strident sound (such as that of chains dragged across stone), and a muffled sound (such as that of a lamp falling onto a rug). An archetypal example of English apophony is the nickname Poll(y) for Mary. Here, excluding the diminutive suffix, we find that every phoneme has been shifted: the occlusive onset (as in pa vis-à-vis ma) connotes the relative masculinity of prepubertal girlhood; the low vowel nucleus (as in stomp vis-à-vis step) connotes the clumsiness of immaturity; and the lateral coda (as in Hal vis-à-vis Henry) connotes affectionately regarded diminutive status.

Even among linguists, the only type of apophony that is widely recognized is vowel gradation. Yet consonant gradation not only exists but occurs in series that are frequently longer than those found in such vowel sequences as occur in tittle ~ tattle ~ stutter. Conspicuous examples are of a type that I call "vertical" apophones, since they range from high, or minimally sonorous, consonants (such as voiceless stops) to low, or maximally sonorous, consonants (such as voiced glides).[12] A labial sequence, occuring in a group of synonymous kinesthetic frequentatives, follows.

1. p : dripple
2. b : dribble
3. v : drivel
4. w : drool[13]

A partially different labial sequence occurs in the second onset position in a series of rhyme-tags, or echo-compounds:

1. p : hodge-podge
2. b : hurly-burly
3. m : pell-mell
4. w : fuzzy-wuzzy

In the case of rhymes 3 and 4 above, of course, the consonantal apophony is double--serial, in relation to rhymes 1 and 2, but also recitational, as regards the two parts of each compound. (In the

case of pell-mell, the apophonic relation duplicates that of the synonymous nicknames Peg and Meg.)

"Horizontal" apophony, on the other hand, moves from front to back--that is, from labial to velar--as in the case of the initial consonants in the roughly synonymous verb series bash = dash = gash or the final consonants in the wholly synonymous (but now largely obsolescent) verb triad chirp = chirt = chirk. A maximal sequence of this type is provided by the root-final consonants in the four British provincial synonyms craps = crits = crutchings = cracknels, all meaning "chitterlings" or "cooked pig's intestines."

However, while all four consonantal positions--labial, apical, palatal, and velar--are found in such English sequences, only two--labial and velar--alternate with high frequency in allolanguage.[14] Examples are the final consonants in the following pairs: yap = yak, glub = glug, and bam = bang.

Internal English affixes are of two types and two subtypes. The two types are: infixes, which are inserted in bases, adjacent to syllabic peaks; and interfixes, which occur between bases in compounds. The two subtypes of infixes are: prenuclear infixes, which occur immediately before syllabic peaks; and postnuclear infixes, which occur immediately after syllabic peaks.

Examples of prenuclear infixes are:

/l/, as in bleep ~ beep
/r/, as in crackle ~ cackle
/y/, as in mew ~ moo
/w/, as in twitter ~ titter

Examples of postnuclear infixes are:

/l/, as in yelp ~ yip
/r/, as in purp ~ pup
/m/, as in tamp ~ tap
/ŋ/, as in clink ~ click

Examples of interfixes are:

/ə/, as in pit-a-pat
/əm/, as in thingumbob
/mə/, as in rigmarole
/əmə/, as in dingumajig

Generally speaking, external affixation in English is a microlinguistic marker, prefixes like the negative un- or the deverbative -ation being absent from allolanguage. Yet a few external affixes not only occur in allolanguage but are characteristic of it. The most striking of such allolinguistic markers in

English is the affix which we may schematically designate by means of the metaphone {Q}[15], since it may be labial, palatal, or velar, voiced or voiceless, occlusive or affricate. Moreover, it may occur either alone or with a vowel, sibilant, or non-lateral lingual and either prefixed or suffixed to a base. Examples follow:

phonemic shape of affix	Q-affixed form	source or cognate form
pɨ	pizazz, "zest"	zazzle, "sex appeal"
pɨr	persnickety, "fussy"	nocky, "silly fellow"
kə	kathob, "vague object"	thob, "be credulous"
kɨr	kerplunk!	plunk, "to drop"
ci	chewallop!	wallop, "to hit"
bə	baluke, "worthless person"	luke, "nothing"
bɨr	bergoo, "porridge"	goo, "slime"
gə	gazook, "a tramp"	zook, "prostitute"
gɨr	gerdoing! "crash"	boing!
p	chirp, "twitter"	chirr, "stridulate"
əp	trollop, "prostitute"	trull, "harlot"
k	puke, "vomit"	spew, "spout"
ək	slummock, "stumble"	slump
əks	lummox, "lout"	lump
c	belch, "eructate"	bellow

In English microlanguage, word-derivation is an almost exclusively expansive process, in which bases maintain their shape while external affixes are added to them, as in the case of kind > unkind > unkindness. Exceptions, such as bite > bit or man > men, occur solely in fossilized paradigms.

In allolanguage, however, derivation may occur with equal frequency by either expansion or contraction. This "accordion" principle is most evident among contemporary nicknames, where the general rule is that names which end in -y are rendered hypocoristic by dropping that ending, while those which do not end in -y are rendered hypocoristic by adding it. Familiar examples are Mare from Mary and Johnny from John. But the accordion operates with equal frequency outside the onomastic realm. Thus we find the expansive verb sequence quell > quelch > squelch side by side with the contractive noun sequence hocus > hoax > hoke. In some cases, allolanguage provides both expansive and contractive derivatives. An example is the verb (s)quash, which may be expanded to "squarsh" or contracted to cosh. In other cases, the direction of derivation

of derivation is uncertain. Thus, any one of the three forms chip = chirp = chirrup may be the source of the other two.

The unpredictability of allolinguistic structure extends from direction of derivation to source of derivation. While uncertain or disputed etymologies occur with disconcerting frequency in microlanguage, they occur with overwhelming frequency in allolanguage. Thus, for example, it is unclear whether the verb glint comes from the base of the verb glimmer by dental stop suffixation (as in snort < snore), from the base of the verb glitter by nasal infixation (as in dandle ~ diddle), or from both bases at once by blending (as in clank < clang X clack). The same problem exists with regard to dolt (vis-à-vis dull and dote), blab (vis-à-vis bla and babble), and whomp (vis-à-vis wham and whop). An archetypally problematic form is the verb slurp, which may be an infixed variant of sup, a suffixed variant of slur, an apophonic variant of slop or lap, or any combination of these.

As the phrase "accordion principle" suggests, there is a strong ludic element in all allolanguage, including at least a latent component of verbal sleight-of-hand. In addition, however, to its teasing quality, linguistic play also has an esthetic aspect. Palimphony and apophony, for example, may quite justly be equated with theme and variation in music, whether that music is vocal or instrumental in nature. And, as Jan Huizinga argues,[16] all art may be viewed as heightened play, whether what is being played is Prince Hamlet or first violin.

My characterization of allolanguage as consisting at least partially of verbal prestidigitation is a deliberate effort to call attention to the pronounced--and often exasperating--strain of elusiveness that runs through most of it. This elusiveness has led many perceptive observers to deny the very existence of certain types of allolanguage, at least in some languages. Thus, as William Samarin notes,[17] those graphic and quasi-sentential utterances which Africanists call "ideophones" are held by many of them to be absent from European languages, despite the fact that almost perfect analogs for them can be found in the languages of every continent. This sort of denial is the linguistic equivalent of the psychological mechanism which leads people of nearly every tribe or nation to ascribe exotic sexual or magical practices to foreigners or the insidious influence of foreigners.

The transparent falsity of the assertion that African "ideophones" are absent from European languages is revealed by a cursory comparison of English with Bini, a Congoid language related to Yoruba and Swahili, whose areal and genealogical relation to non-African languages is minimal at most. In both languages, ideophones (alias allolinguistic forms) exhibit marked palimphony. Two types of palimphony shared by Bini and English are assonance and triplication. Examples of the former are Bini zàghàzàghà, "sparse and patchy," and English ramshackle; of the latter, Bini wẹ̀wẹ̀wẹ̀,

"whispering," and English wee-wee-wee (describing the squeal of a piglet).

Another allolinguistic trait shared by the two languages is dysphony, or violation of the phonic canons of their respective microlanguages. Three types of dysphony which they have in common are: abnormal vowel length, xenophony (use of foreign speech-sounds), and infantilism (reduction of the normal phonemic inventory by avoidance of distinctively "adult" speech-sounds). Examples of abnormal vowel length are Bini kpŭ, "plop!", whose vowel is only half the length of ordinary syllabic peaks, and English boo-oo-ool, a jeer whose protracted vocalism clearly differentiates it from the ejaculation boo!, used to startle persons mistakenly imagining themselves to be alone. Examples of xenophony are Bini jǫjǫjǫ, "jingle-jangle," and English [y ɨ + x], an exclamation of disgust, which may be apophonically related to the canonical forms icky and ugly. (The voiced prepalatal affricate in the Bini form is typical of Yoruba, and the voiceless velar fricative in the English form is typical of German.) Examples of infantilism are the Bini address-form bàbá, "daddy" (as against its adult equivalent èβàβá, "dad") and the English nickname Ted (as against its formal equivalent Theodore).

A third allolinguistic trait shared by Bini and English is dysmorphy, or use of grammatically uncanonical forms or devices. A type of dysmorphy common to the two languages is morphological reversibility, whereby bipartite forms may occur indifferently in the order AB or the order BA. A Bini example is the modifier kángún = gúnkán, "lean"; an English example is peep-bo = bo-peep, the nursery form of a children's hiding game.

A fourth allolinguistic trait shared by the two languages is employment of morphological markers to indicate that the forms to which they are attached are not microlinguistic. Such a marker is an infix employing the labial nasal, as in the Bini ethnic slur-word ìɣònmíìɣòn, "northerner" or "outsider," or the English comic name Hod-ma-Dod (probably standing for Roderick).

Another subject of uncertainty is that of the status of those entities which Fred Householder calls phonesthemes.[18] That few linguists are comfortable with phonesthemes is amply attested to by the fact that the term does not appear in the indices of most standard works on linguistics. Those few linguists who do deal with phonesthemes tend to define them as submorphemic phoneme-clusters having (at least connotative) meanings distinct from those of the morphemes in which they occur. The phonesthemes most widely recognized are initial diphonemic clusters like the gl- in glow, gleam, and glisten. Perhaps because of the prevailingly synchronic character of linguistic research during the past half-century, some who cite this and similar phonesthemes seem unaware of the fact that, historically at any rate, the gl- here is a zero-vowel allomorph of the morpheme that occurs vocalized in the words gold,

yolk, and yellow. Etymology, however--a fortiori in allolanguage--has little effect on function.[19] Consequently, phonesthemes cannot be satisfactorily disposed of simply by explaining them away historically.

Many phonesthemes, moreover, cannot be identified as allomorphs of recognized morphemes. Among those phonesthemes which resist such identification are the non-initial vowel-plus-consonant sequence in verbs like hack, smack, and crack and a group of monophonemic segments that may occur equally well in initial and in final position. Of these minimal segments, the most conspicuous is /z/, which, as an allolinguistic marker, first appeared in early Modern English and which may occur in any of four positions, here listed in order of earliest attestation.

1. (16th century) stem-final position: drizzle
2. (17th century) word-final position: buzz
3. (18th century) word-initial position: zigzag
4. (19th century) stem-initial position: kazoo

The z-phonestheme also occurs in palindromes, like the slang term zizz, "sleep." The relative rarity of this phoneme in pretonic microlinguistic position and its relative frequency in racy speech of various kinds constitute strong evidence for the view that it should be regarded as having an unusual semantic function.

A more specialized kind of allolinguistic form consists of a lexemic type which, so far as I know, linguists have not named because they cannot agree even on its existence.[20] Since it invariably contains five and only five phonemes, I have christened it the "pentestheme." In its more inclusive formulation, it consists of the sequence obstruent-plus-sonorant-plus-monophthong-plus-continuant-plus-obstruent. So defined, it includes such words as grasp, slurb,[21] and swoop.[22] In its less inclusive formulation, it consists of the sequence obstruent-plus-lingual-plus-monophthong-plus-nasal-plus-(voiceless) stop. So defined, it is confined to words like plunk, crimp, and shrink. (In the latter case, inclusion or exclusion of words like cringe and crunch depends on whether their terminal affricates are classified as units or as clusters.)

The probable reason for the distinctiveness of the pentestheme in English is that it represents the basic CVC root-shape[23] amplified by the maximum number of infixes that English allolanguage permits. The root of bubble, for example, can be enlarged by prenuclear infixation, as in blub; by postnuclear infixation, as in the stem of burble; or by double infixation, as in blurb. According to Morris Swadesh,[24] infixed bases of this kind are primordial both in fact and in feeling and so elicit the kind of ambivalent fascination that our jaded mentality exhibits toward the "primitive."

Among other difficulties with the whole concept of allolanguage, of course, is that, like the term "invertebrate" in zoology, it is an

intrinsically negative category. It defines itself chiefly in opposition to microlanguage. Yet invertebrate zoology is a thriving scientific enterprise. Moreover, the highest invertebrates, such as octopuses, are celebrated equally for their relatively large bulk, their complex eyes and brains, and their almost incredible skill in eluding their captors by squeezing out through tiny cracks in holding tanks. In their autocentric devotion to vertebrates, it may be that biologists are overvaluing backbones in much the same way in which linguists are overemphasizing microlanguage. As kinesicist Ray Birdwhistell once put it to me in an informal conversation at an anthropology meeting, we are unlikely to understand human evolution in its entirety until we learn to pay as much attention to soft parts of the body as to hard. Paraphrasing this observation in linguistic terms, I would say that we are unlikely to understand human communication in its entirety until we learn to value the "soft, warm facts" of language as much as the cold, hard ones.

The elusiveness that we have noted in such allolinguistic phenomena as ideophones, phonesthemes, and pentesthemes mirrors, in some regards, the elusiveness of such paranormal phenomena as clairvoyance, precognition, and telepathy. In at least some of the glossas, which include tongue-speaking, scat-singing, and nonsense-verse, trance has been reported on the part of the vocalizers involved.[25] Furthermore, many allolinguistic utterances involve synesthesia, at least insofar as synesthesia involves simultaneous reference to two or more different sensory modalities. And synesthesia in its strictest form--such as seeing music or hearing colors--is a typical manifestation of altered, or non-waking, consciousness. Examples of synesthetic forms, selected exclusively from the domain of frequentative verbs are: sparkle, which is at once visual and attitudinal; splutter, which is at once auditory and kinesthetic; and tickle, which is at once tactile and attitudinal.

As linguists normally use the terms underground and substratum (to describe criminal speech-communities and dead languages that affect successor languages, respectively), we can hardly equate either one with allolanguage. But, in another and more psychoanalytic sense, both terms do describe allolanguage. For both graphically suggest the simultaneous suppression and repression to which allolanguage is subject--as manifest in the facts that most Africans will not write ideophones (though their speech is heavily laced with it) and that some linguists deny the very existence of such phenomena as pentesthemes. The various deformations and disguises that Freud posited as characteristic of dream-structure are neatly parallelled by the phonic rearrangements in such linguistic euphemisms as gad, gosh, cor, 'Od, and--perhaps most revealingly--phonic zero (as in 'zounds) for God.

The timelessness of the unconscious mind seems equally evident in the simultaneous evanescence and immemoriality of allolanguage, where items seem as short-lived as subatomic particles, while patterns

seem as ageless as megalithic monuments. The coexistence of contradictories which psychoanalysts also find in the unconscious is likewise echoed in allolanguage, where items like kerflooey have no opposites and rarely occur in negative sentences.

The repetition-compulsion is also evident in allolanguage, where palindromy, rhyme, reduplication, and similar devices serve to mark forms as other than microlinguistic. Another characteristic of unconscious mentation is overdetermination, or production of symbols from a greater number of sources than is strictly necessary. Its allolinguistic equivalent is polygenesis, or derivability of secondary forms from a plurality of primary forms. A representative polygenetic product is the (once hypocoristic) surname Bibby, which may be derived by vowel apophony from Bob(by) or Babs, by palindromy from Bill(y) or Lib(by), or by a blend of any or all of these--not to mention possible connections with the common noun bib, the surname Beebe, and the place-name Bebington.

A particularly striking case of linguistic overdetermination is provided by a family of echoic palindromoids (that is, words whose bases are palindromic at the feature level if not at the phoneme level--in which terms, all pentesthemes are palindromoids). This family, whose core consists of a syllable which may be metaphonically transcribed as {KAK} and glossed as "vocalization," has over 100 English representatives. Among the more familiar of these are: cackle, cuckoo, quack, cluck, croak, squawk, screech, giggle, chuckle, shriek, and honk. They are overdetermined in the sense that they not only employ vocalization to represent vocalization--as do caw, coo, and the like--but, by repeating base-onsets as base-codas, mimic themselves. In this sense, they are doubly duplicative.

Blending is a far more frequent device in allolanguage than in microlanguage, whether it occurs anonymously, as in the case of bash (from bat X mash), or in a documented manner, as in the case of Lewis Carroll's celebrated coinage chortle (from chuckle X snort). Its psychodynamic equivalent is the mechanism that Freud called condensation and found most frequently in composite dream-characters, while Jung also found it in the chimeras of mythology, such as griffons and sphinxes.

Allolanguage is far more difficult to paraphrase than is microlanguage. Were this not so, allolanguage might by now have replaced it, at least in the literate industrialized world. But its very resistance to discursive formulation is probably what puts allolanguage on a par, in terms of its ability to satisfy imperfectly articulated human needs, with day-dreaming, with game-playing, and with the fine arts.

The polymorphy of allolanguage, due largely to the freedom with which it generates and absorbs neologisms, recalls the "polymorphous perversity" of the erotic impulse, which Freud saw as a blight on human sexuality, while Norman O. Brown saw it as a blessing.[26]

This overview of allolanguage, sketchy as it must necessarily be, inevitably raises more questions than it answers. One of these is the question how many of the allolinguistic forms and devices recorded in our permissive age may have existed but gone undocumented in earlier times. In particular, I wonder about Old English allolanguage, knowing that, before Chaucer's time, very little of our language got recorded that was not literary, ecclesiastical, or administrative in nature. My own suspicion is that the once unattested suffix that appears in Modern English dollop, "blob," did exist as an augmentative suffix in Old English allolanguage. The few etymologists who recognize the suffix at all derive it (in words like scallop) from Old French. However, the fact that these French words come from Frankish indicates that the suffix was early Germanic. And the semantic opposition between such 17th and 18th century nouns as tit, "girl," and tittup, "huzzy," further suggests augmentation (in contrast to the still diminutive force of the corresponding velar suffix in bullock vis-à-vis bull).

I further suspect that the consonantal onsets [dž] and [z], as in contemporary jabber and zonk, also occurred in Old English allolanguage. To those who object that these un-Anglo-Saxon phones were exclusively Medieval French importations, I (somewhat hesitantly) reply that the speakers of early English probably would not have accepted such microlinguistic foreignisms at all had they not already been exposed to them in "underground" speech. And doublets like jillion = zillion suggest that these two exotic onsets continue to have allolinguistic potency.

Another such question is whether one may not justifiably expect that any units and processes which are dominant (i.e., microlinguistic) in one language will invariably turn up in other languages, if not as dominant then at least as recessive (i.e., allolinguistic) elements. A notorious example of this principle is the supposed uniqueness of click consonants among the Khoisan languages of South Africa. While it is true that no other language phylum is known to employ clicks within nouns and verbs, many others use clicks as expletives. We ourselves, for example, employ clicks to call pets, encourage horses, and express disapproval.

Yet another such question is whether, as the cognitive and emotional tone of allolanguage suggests, it is neuroanatomically as well as psychologically divergent from microlanguage. More precisely, is allolanguage a product of the right cerebral hemisphere (or "intuitive" forebrain), as art and interpersonal skill seem to be, rather than of the left cerebral hemisphere (or analytic forebrain), as logic and microlanguage seem to be? Before this question can be answered, not only will neurophysiologists have to learn more than is now known about brain function, but linguists will have to evince more interest in the subdomains of macrolanguage than they have thus far exhibited.

My final--and no doubt most crucial--question has to do with the future of allolanguage: Is it dying? Will it soon be entirely replaced by microlanguage? The question is a reasonable one, in view of the fact that the exclamatory and imitative components of allolanguage are almost certainly pre-human and that microlanguage is now sharing our communicative load with allolanguage. I hope I may be forgiven if I do not attempt a predictive answer but confine myself instead to an expression of sentiment: If microlanguage is the light of human linguistic behavior, allolanguage is its lustre. I fervently hope that allolanguage will continue to enrich and perplex us for a long time to come.

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[4]Chomsky, Noam: Language and the Mind, Harcourt, Brace, and World, New York, 1968, passim.

[5]Swadesh, Morris: "Lexico-Statistic Dating of Prehistoric Ethnic Contacts," Proceedings of the American Philosophical Society, v. 96, 1952, pp. 452-563.

[6]Wescott, Roger W.: "Labio-Velarity and Derogation in English: A Study in Phonosemic Correlation," American Speech, Spring, 1971, passim.

[7]Diffloth, Gerard: "Notes on Expressive Meaning," Papers of the Chicago Linguistic Society, April, 1972, pp. 441-445.

[8]Wescott, Roger W.: "English Palindromes as Linguistic Icons," a paper presented at the Annual Conference of the International Linguistic Association, New York, March 11, 1972.

[9]Rather than palindromization, this may be a case of labial/velar alternation. See footnote #6, with special reference to p. 124 of the article cited.

[10]There is no argument about assonance in compounds like tid-bit, where monophthongs are followed by non-glide consonants. But, for those who employ a Tragerian phonology, sequences like flea-bitten are arguable, since their syllabic peaks are identical although their syllabic nuclei are not.

[11]Wescott, Roger W.: "Word Chains in English," Verbatim, May, 1974.

[12]Wescott, Roger W.: "Apophony in Proto-Speech," in Language Origins, edited by Roger W. Wescott, Linstok Press, Silver Spring, Maryland, 1974, pp. 140-143.

[13]Here we assume a Middle English etymon *driw(e)len. For the abridged Modern English frequentative suffix, compare mewl from mew.

[14]See fn. 6, with special reference to p. 124 of the article cited.

[15]A metaphone is a morphophoneme or any categorially comparable class of phonemes. See Roger W. Wescott, "Metaphones in Bini and English," in Studies in Linguistics in Honor of George L. Trager, Mouton, The Hague, The Netherlands, 1973.

[16]Huizinga, Jan: Homo Ludens: A Study of the Play Element in Culture, Routledge and Kegan Paul, London, 1949.

[17]Samarin, William J.: "A Survey of Bantu Ideophones," African Language Studies, v. 12, 1971. In his own view, however, language of the ideophonic type is a linguistic universal.

[18]Householder, Fred W.: "On the Problem of Sound Meaning: An English Phonestheme," Word, v. 2, 1946, pp. 83-84.

[19]Bolinger, Dwight L.: "Rime, Assonance, and Morpheme Analysis," Word, v. 6, 1950, p. 119.

[20]See Robert A. Hall, Jr., Introductory Linguistics, Chilton Books, Philadelphia, 1964, p. 185 (on "sound symbolism . . . and . . . onomatopoetic words").

[21]Slurf is an allolog of slurp. Compare the British slang term bumf, "toilet-paper," vis-à-vis the American slang term bumpus, "buttocks."

[22]Here we presuppose a Tragerian phonemicization of swoop as /swuwp/.

[23]Wescott, Roger W.: "The Origin of Speech," in Language Origins, edited by Roger W. Wescott, Linstok Press, Silver Spring, Maryland, 1974, pp. 113-115.

[24]Swadesh, Morris: The Origin and Diversification of Language, Aldine-Atherton, Chicago, 1971, pp. 110 and 134.

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THE NEED FOR A SOCIOLINGUISTIC MARKING SYSTEM: AND A PROPOSAL

Jacob Ornstein
University of Texas, El Paso

A. Background and Rationale

In a world where new panaceas and models, economic, ecological and linguistic proliferate, the need for still another scheme of any sort would appear superfluous. Nevertheless, the writer seeks justification in the knowledge that here is seemingly a complete lacuna as regards exclusively sociolinguistic marking systems. Consequently little time need be wasted in wrangling about the comparative merits of competing plans.

At the 11th International Congress of Linguists, Bologna, 1972, in a paper titled "Sociolinguistic Changes Viewed Within a Tagmemic Framework" (now available in the Proceedings (Heilmann: 1975: 199-219), the author submitted a Tentative Sociolinguistic Notational System. A somewhat revised version of the above essay is also due to appear in New Horizons in Linguistics (Makkai and Heilmann; forthcoming). It should be insisted, however, that the topic, in our view, transcends the matter of linguistic models so that the tagmemic framework merely reflected our special interest in this orientation at my home institution.

Our concern is with the difficulties involved in identifying the sociolinguistic status of isolates occurring in an oral or written continuum, that is, in the surface structure of language. In ordinary linguistic and language writings, it is too commonly assumed that readers (or listeners) have a native speaker's awareness of the regional, social or other features of a lexeme or item, for want of a better term. Even native speakers cannot be aware of all significant variations in their own language, just as one may encounter educated Americans who do not know that tonic means "soda pop" in New England. Older ladies speak of a former beau, a term well into obsolescence as far as persons under forty are concerned. The adjective nice in some varieties of Black English has a deviant sexual connotation. We are then speaking of items that are somehow "marked", and may call attention, eliciting surprise, or raising doubts about meaning or appropriateness. In other words these depart in some way from an undisputed standard base, in line with the concept of American Network English.

The fact that there has been no race to produce sociolinguistic marking systems is, in our mind, a reflection of what might be called the "unitary" fallacy, endemic in those who work with

language, be it in a theoretical or applied capacity, and who are aided and abetted by the pre-linguistic thinking of laymen in their quest for that one and only "correct" form. Chomsky has time and again been "roasted" for his preoccupation with the speech of "an ideal speaker-listener in a completely homogeneous speech community who knows the language perfectly." (1965:3) Nevertheless, he should not be made the sole scapegoat for a syndrome common even among the most ardently self-avowed anti-prescriptivists of whatsoever linguistic persuasion.

Fortunately the recent upsurge of interest in language variation is promising, since it is being discovered that in human language diversity may indeed be more common and typical than a putative homogeneity. Indeed in his book Bilingualism as a World Problem, William Mackey (1968: 11) asserts that there are more areas on the globe characterized by diversity than by homogeneity. This is by no means negated by the existence of such strikingly monolingual lands as Iceland, Uruguay or Australia, since even there infra-language diversity is a function of social class. Since all individuals belong to definite sub-groups within the macro-society called nation, differences are implicit. The fact is that as soon as a person produces an oral or written string of speech, he marks himself sociolinguistically -- that is, signals the nature of his group affiliations. What is lacking is a set of widely accepted discrete symbols, which would supply supplementary data, mostly of extra-linguistic type, to those remote from such speakers and writers. Viewed in this broad frame of reference, nothing less than a sociolinguistic notational system is called for.

At this point angry looks from lexicographers in our midst would be understandable. As a matter of fact, the insights of such individuals as Lawrence Urdang, editor of the Random House Dictionary (and a Charter member of LACUS) are badly needed for the problem under discussion, as is the experience of other scholars involved in research-based corpora of languages. W. Nelson Francis, compiler of the Brown University Standard Corpus of the American Language, in a detailed and perceptive discussion of English projects of this sort notes that:

> ...a great deal more could be done easily with a corpus if it were tagged in some way, by grammatical abbreviations following each word, by subscript numerals, or whatever -- some kind of overt mark which the computer could use to put together what should be together and to separate what should be separated, functions which the reader does on the basis of the whole structure and context of the utterance. (1975: 36)

Although Francis is limiting himself largely to the grammatical dimension, this suggests that linguistic and lexical corpora generally suffer from inadequate marking, despite the fact that most

dictionaries and word lists employ a limited number of abbreviated status-markers to indicate, for example "regional," "colloquial," "archaic," "obscene," or even "non-standard." Quite understandably, however, there is wide variation among all these, both as to the manner and extent of what is flagged. At best any marking system faces numerous and subtle complexities. Many of these are pointed up by David Gold in his essay "New Perspectives in North American Lexicography" (1975: 21-29) In addition to the difficulties of defining "word" itself, which he regards as a "folk unit with no cross-cultural validity," he notes the pitfalls dealing with lexemes separated from their context:

> Of course, there are distinguishable levels of usage, but in situations which words vary from one extreme to another, attempting to fix them on one level without context would inevitably result in being more misleading than helpful.

Be all this as it may, the Bologna paper and its revision both contain fairly full reviews of systems which are somehow related to the problem of flagging sociolinguistic status, so that this material need not be repeated here. In any event, most of these have other aims, such as the gross classification of language varieties, rather than concern with specific items, be they of phonological, syntactic, lexical, semantic nature, or otherwise. This is admittedly a large order but as Max Millikin of MIT has said of economics, linguistics, too, must depend on a series of "successive approximations" to advance from the status quo.

B. Problems of Marking in a Multilingual Area

Interest in this topic has been triggered by the author's decade of teaching and research in two of the five states of the U.S. Southwest -- the nation's largest area of ethnolinguistic diversity. In particular, work on this University's corpus of the Sociolinguistic Studies on Southwest Bilingualism (SSSB), an activity of the Cross-Cultural Southwest Ethnic Study Center, has constantly confronted him and other colleagues with a continual series of puzzling problems regarding the status of the language varieties in contact and in conflict (cf. Ornstein, 1971).

In the West Texas area, a minimal count of language varieties would have to include the following: Standard Mexican Spanish, Mexican-American or Southwest Spanish, with its own norms influenced by general English interference, and Mexican-American English, marked especially by English phonological interference. The latter two fully merit Einar Haugen's classification as "bilingual dialects" (1969: 72: 370-71) or "contactual dialects" (1970: 19-26). In addition two other varieties should be noted. One of these is a Spanish-English pidgin used for superficial business transactions, and in interaction with maids and workers. The other is a considerably stigmatized calo or argot known mainly as Pachuco, of semi-

delinquent origin. In common with such underworld codes as the speech of American gangsters and the Parisian Apaches, much of its lexicon, rich in imagery, has also been incorporated into standard informal varieties of Spanish. It is, therefore, not surprising that even well-informed Southwesterners, be they monolinguals or bilinguals, often lump all these varieties together pejoratively under the term "Tex-Mex", "Border Lingo" or "Pocho" (the latter term recently acquiring increasing respectability). Returning to the English of the area, this is a dialectologist's nightmare, with a spectrum present of Southwestern sub-dialects of basically Northern Midland and Southern Midland origin, reflecting the highly mixed pattern of in-migration.

The language situation is even more complex as one moves north and west of here. There are hundreds of Amerindian languages, and numerous pockets of trilingualism. An example is that of Pascua, a Yaqui-Spanish-English speaking community outside of Tucson, Arizona, described by Carroll Barber (1973: 295-320). Throughout the Southwest there are also several dozen European and Asian languages represented, of both older and recent immigrations, particularly in the larger towns and mining settlements. The bilingualism of such groups as the Italians, Syrian-Lebanese, or Hungarians is typical of the American historical pattern, and of dynamic or evanescent type, with language maintenance eroding within one generation. These immigrant communities lack the socio-cultural-linguistic matrix afforded Spanish-English bilinguals, thanks to the 2,000 mile long international frontier. This has had the dual effect of promoting Spanish language maintenance and of preventing true creolization from occurring. Reservations and communal structures have served a similar purpose for Native Americans. It is for this reason that several social scientists here (De La Garza, Kruszewski and Arciniega, 1973: 97) have coined the term "territorial minorities", to characterize the uniqueness of the socio-cultural and linguistic position of Chicanos and Amerindians.

Consequently, the very number and intensity of language contacts generates problems of status beyond mere standardness vs. non-standardness, although this dichotomy also looms large. One thorny issue is the degree of integration of an item into the bilingual dialect or into Standard Mexican Spanish itself. An illustration is troca "truck" which many bilinguals and Spanish teachers themselves condemn as corrupt "Tex-Mex", and obviously non-standard. The contrary is, however, the case since camión the normal term in most Spanish-speaking lands, has acquired the meaning of "bus" in Mexico and the Southwest, the gap being filled by English "truck", rephonemicized into its present form. Along with this there are subtle distinctions to be noted in code-switching behavior, (cf. ongoing work by Guadalupe Valdés-Fallis, 1974), involving whether a lexeme, idiom or string is an outright English foreign loan, or whether these have been integrated into the Southwest Spanish bilingual dialect. As Gumperz and other sociolinguists have observed language contact generates its own norms

of bilingual behavior (1969: 435-49). Nowhere is this more apparent than in technical domains, so that for instance, lexemes having to do with autos, tractors and other equipment, their repair and maintenance, are almost exclusively integrated features* or loans. The same is true of numerous household terms; those employing standard dictionary terms other than mapa "map", sink "sink", klineks "paper tissue" simply run the risk of not being understood. It is problems like these, treated haphazardly at present, which our marking system attempts to address.

In a previous study, "Toward a Classification of Southwest Spanish Non-Standard Variants", presented at the 1972 Summer Meeting of the Linguistic Society of America (1974), the writer devised a taxonomy, consisting of a four-way system of dual slots concerned primarily with whether non-standard variants co-occur i.e. compete or not with prestige standard equivalents, in both monolingual and bilingual communities, or exclusively one of these. In this connection, a common fallacy is that numerous non-standard lexemes developed in the Southwest and are "typically" of "Tex-Mex" nature. The schema in question is, however, limited mostly to bilingual/bidialectal contact situations, hence not broad enough for an overall system.

C. An Approximation of a Notational System

An ideal sociolinguistic flagging system would combine an economy of symbols and of meaningful oppositions. As sociolinguists like to point out, the idiolect of most speakers represents a continuum of both standard and non-standard features, although the clustering of one or the other type toward either pole of the spectrum may serve to identify his general pattern.

The schema to be presented below is basically a digraphemic one. To the extent possible, the first two letters of a one-lexeme term, or the first letters respectively of a two-lexeme one serve as the symbols. This has obviously had to be waived where ambiguities might occur and a triliteral one sought. Even this was insufficient in such a case as "interferential", so that the solution was sought in choosing a letter, with some phonological suggestivity, from the interior of the word, resulting here in "if". As much as possible, terms already in wide currency, such as st for "standard", were simply adopted, as was SES for "socio-economic status", frequent in social science writings.

One of the most formidable difficulties in attempting to come up with meaningful labels stemmed from extreme relativity. Once attempts were made to apply them to actual examples from the real world of language in use, rather than some logico-mathematical

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*In this paper "feature" is utilized within our own classificatory framework and not in the sense, let us say, of "distinctive feature."

vacuum. The same is true of the quest for significant dimensions or groupings to which the labels may correspond. As may be seen, the smallest number of symbols fall under the Standard dimension or rubric, a somewhat larger one under Non-Standard, and the preponderance under Neutral. The latter is still a rather inchoate category in need of pruning and further sub-division. A fourth dimension, Situational Factors, aims at pinpointing significant features of the participants of the speech act and its realization. It includes, among other things, oral or written delivery, tempo of delivery, nature of the interlocutors, setting, atmosphere (friendly or hostile), basically falling under Dell Hymes concept of the "ethnography of speaking". Particularly in this grouping, use is encouraged whenever necessary, to use an explicative device including the abbreviated symbol, a plus sign, followed by brief detail within parentheses, as in the factor "age": ag + ( ). Further examples of application may be seen in II of the schema itself.

In order to go beyond its present stage, the schema needs further refinement, and especially application by colleagues. It is our feeling that this, or any other marking system, needs to avoid excessive proliferation, lest it become in itself an unwieldy superstructure upon the structure of language itself. Moreover its use would probably be most effective if it is as sparing as possible.

Accordingly here is the schema revised slightly since its presentation at Bologna:

A TENTATIVE SOCIOLINGUISTIC MARKING SYSTEM

I.

SYMBOL	(According to Dimension) EXPLANATION	MARKEDNESS
ST	STANDARD	-
fo	formal	-
pr	prescriptive	-
no	normative, often of "school variety"	-
sa	sanctioned by recognized authority (academy, etc.)	-
of	official	-
na	national	-
re	religious	-
ac	acrolect (al), higher prestige variety [1]	-
sti	stilted	-
ru	rustic, rural	+
NS	NON-STANDARD	+
st	stigmatized	+
ar	argot, caló	+
pa	patois	+
ja	jargon, specialized code	+
ba	basilect(al), lower prestige, stigmatized variety[2]	+
sl	slang	+

SYMBOL	(According to Dimension) EXPLANATION	MARKEDNESS
ca	cant	+
cr	creole	+
pi	pidgin	+
cd	contactual or bilingual dialect,[3] interferential variety	+
if	interferential feature or item,[4] usually not assimilated within bilingual community	+
af	assimilated feature[5] or item, integrated within bilingual community	+
lo	loan, feature or item often not part of standard language[6]	±
hy	hypercorrect, to point of non-standardness	±
NE	NEUTRAL (STANDARD OR NON-STANDARD)	±
la	language	±
di	dialect(al)	±
le	lect, isolect (replacing dialect?)[7]	±
va	variety	±
ve	vernacular, "unmonitored" style of speech	±
sc	specialized code, e.g. teenagers, occupational	±
sty	style (write: sty + (des.)	±
co	code (write: co + des.)	±
reg	register (write: reg + des.)	±
re	regional, geographically determined. Write first 2 letters of country or region, regist 3 of <u>la</u> or <u>di</u>, e.g. FR = France; FRE = French)	±
so	social	±
vu	vulgar	±
po	pornographic	±
ob	obscene	±
obs	obsolete, obsolescent	±
arc	archaic	±
hf	high-frequency item or feature	±
lf	low-frequency item or feature	±
wp	word-play, usually involving transposed phonotactics, e.g. Pig Latin	±
ta	taboo	±
su	status uncertain, lacking further data	±
in	informal	±
un	unstable, fluctuating, neologistic, disappearing, etc.	±
ss	semantic shift, often away from ST	±
ch	changing, shifting; indicate direction with arrow	±
pe	petrified, fixed, often archaic form	±
qu-	quasi	
SI	SITUATIONAL FACTORS OR DIMENSION	
SES	socio-economic status (of speaker)	

SYMBOL (According to Dimension)	EXPLANATION	MARKEDNESS
ag	age, age group. Write: ag + (des)	
se	sex. Write: se + (des)	
co	context. Write: co + (des)	
au	audience, interlocutors. Write: au + (des)	
at	atmosphere. Write: at + (des)	
is	isolation from "mainstream". Write: is + (des)	
et	ethnicity, affiliation to ethnic group. Write: et + (des)	
ll	language loyalty. Write: ll + (des)	
or	primarily oral usage	
wr	primarily written usage	
te	tempo, speed of delivery. Write te + (des)	
no	notabilia, special circumstances or details to be noted. Write: no + (des)	

II. Examples of Use

1. CLAUSE LEVEL

(1) (ST,fo) Había + muchos hombres ± allí.
Pr exist; Nom Comp; Loc

"There were many men there"

(2) (ST,in) Habían + muchos hombres ± allí.
Pr intr(x); Subj pl; Loc

2. PHRASE LEVEL

(3) (S,fo,SP) un poco de agua
Det + Quant + Prep (N)
x1 x masc de x fem

"A little water"

(4) (S,in,ME) una poca de agua
Det + Quant + Prep (N)
x2 x fem de x fem

3. WORD LEVEL

(5) (ST,fo) siénten / se
Verb imper Refl pron "Sit down (pl)"

(6) (NS,ba,US_{sw},ME) siénten / se / n
Verb imper Refl pron Person marker

(7) (qu-ST,inf,ME,di,US_{sw}) droga "debt"

(8) (NS,cd,ss,hf,US_{sw},SPA ⟵ Eng. attend) atender "To attend"

(9) (NS,cd,ss,jo?,su,US_{sw},SPA ← Eng. grocer) grosero "grocery man"

(10) (NS,cd,bl,if,su,US_{sw},SPA ← Eng. advantage + SPA ventaja) adventaja "Advantage"

(11) (NS,cd,lo,af,US_{sw},SPA) /suera/ "Sweater"

(12) (NS,ar-Pachuco,ba,se-M,co-peers,US_{sw},MW) refinar "to eat"

(13) (NS,cd,hf,ba,SPA,US_{sw},ME?PA) mušaša ~ (ST,fo,SES-higher st-careful,SPA,US_{sw}ME,PA) mučača muchacha "girl"

(14) (ST,hf,ag-younger,AR,SPA) kaše ← (ST,ch,ag-older,AR,SPA) kaže calle "street"

NOTES

1. The term "acrolect" and its opposite "basilect" have been contributed by Stewart (1968).

2. "Street variety" was suggested to me by Charles G. Elerick, of this University (Ornstein 1971: 357).

3. "Contactual dialect" was coined by Haugen (1971), although he had previously suggested "bilingual dialect" (1969; 72; 370-71).

4. The dichotomy of "interferential" versus "integrated" loans within a bilingual speech community is a concept of Mackey (1962). Nevertheless, rather than "integrated", we have adopted the term "assimilated", following Craddock (1972).

5. The term af "assimilated feature" as well as bl "blend", was adopted from Craddock (1972) in his taxonomy of Southwest Spanish lexicon.

6. The writer has wished to go beyond the merely lexical suggestivity of Haugen's "loanword" and "loanshift" (Haugen, 1956: 55 ff) hence preferring lo "loan feature" and ss "semantic shift".

7. C. J. Bailey, proposes that since "dialect" is unacceptable to so many, it be replaced by "isolect", which is G. B. Milner's term or merely "lect", apparently first used by him and A. A. Afendras (Bailey, 1971, passim).

8. Labov defines vernacular as "the style in which the minimum attention is given to the monitoring of speech." (1970: 46 ff). A broader definition is a language variety in mostly oral use.

It may be added at this point that marking systems would probably have especial utility with "languages of wider communica-

tion" such as English, French, or Arabic. As regards Spanish itself the problems of variation are particularly numerous since it is the official medium of nineteen nations -- more than any other language. For example, a lexeme like coger has throughout the Spanish-speaking world a standard meaning of "to grasp, seize", as well as the obscene connotation "to perform the sex act". Nevertheless, in the River Plate Area (Argentina, Uruguay, and Paraguay) only the latter meaning is in currency.

Again, one of the by-products of working on a system such as this has been to make the writer infinitely more aware of the inextricably intertwined and interlocking nature of any terminology referring to the dimensions of language. Gary Underwood (1974: 40), an atlas dialectologist turned sociolinguist, in a recent writing has pointed up the difficulties of even making a real distinction between "regional" and "social dialects", suggesting that one might even collapse the two into a single term. Perhaps hyphenation is the solution and we may submit "socio-regional" dialect, a coinage apparently originating in this paper. The same, of course, might be said of numerous other concepts.

D. The Possibility of Joint Efforts

Although still impressionistic in nature despite its origin in experience with a bilingual corpus, this schema can perhaps best serve as a point of departure for further efforts, preferably in a joint framework. It is high time for sociolinguistics to break away from its fixation upon the speech of the ghetto and the barrio and to address itself to the language of all socio-economic classes. Edward Blansitt Jr., a colleague at my school, suggests (Personal Communication) that perhaps the principal concern of a marking system might be to help determine more precisely what constitutes standard language in its different contextual varieties, from the elegant style of professional communicators to the vernacular style of comic strips and books.

Our recommendation is that a series of cooperating mini-projects be set up, perhaps under LACUS auspices for those wishing to study usage both in "mainstream" and culturally diverse urban and rural contexts. In so doing, it is not necessary to "invent the wheel all over again" but one can build upon the data and experience gained by Fries, Eagleston, Gove, Urdang, Leech, Allen, Cassidy, Bagby, Francis, Kucera, Twaddell and others in working with corpora of different types. A fine example of such utilization is that of Margaret Bryan's Current American Usage (n.d.), which employed information from the various Linguistic Atlas projects (particularly the Linguistic Atlas of New England), as well as briefer usage studies by colleagues in different parts of the country. This compendium throws light on such alternating patterns as "It's I" vs. "It's me"; can vs. may and sentence modifier patterns. Although compiled before the heavy impact of sociolinguistics, Bryan shows herself surprisingly liberal in what she classifies as "standard informal",

although expressing an awareness that teachers and others dedicated to rigid norms would doubtless be shocked by much of this (xxi-xxiv).

Another useful model has been afforded by Quirk and Svartvik in their work Investigating Linguistic Acceptability (1966). Basically they utilized as subjects their own college students at British universities, presenting them with sets of sentences forcing a choice between variants, such as burned vs. burnt. Much of their research is also incorporated in their new and comprehensive A Grammar of the Contemporary English (Quirk, Greenbaum, Leech, and Svartvik, 1967).

The advantage of procedures like these is that they can be applied literally anywhere with population samples of varying SES, age, sex, ethnicity, isolation and the like. In these days of shrinking material support for the humanities, mini-projects can be carried out with tiny grants (or none), the employment of work-study students, and the involvement of language and linguistic majors and colleagues.

Targets for our surveys, both of short and long term types, are not lacking, although emphasis would be on focussed research rather than dialect atlas type of comprehensive investigation, perhaps feasible only in countries like West Germany and other German-speaking lands where these efforts are underwritten by regularly allocated government funds. In North America, quite naturally, emphasis would be on French, English, Spanish and Portuguese. For those researchers interested in language contact, a fertile field beckons in investigating the newer bilingual and bidialectal communities resulting from the most recent waves of immigration from the West Indies, Latin America, the Philippine Republic, Vietnam, and other Southeast and South Asia nations. Turning to Western Europe, relatively little has been done on the new phenomenon of migrant workers, forming ethnolinguistic enclaves contrasting sharply with possible New World counterparts.

Perhaps there is even a note of urgency about marking systems, after all, the basic theme of our essay. Nelson Francis (27) points out that none of the regional Linguistic Atlas materials have been computerized. If indeed a viable schema might materialize before such storage occurs, much societal and extra-linguistic data could be easily incorporated, rather than remaining with the field workers only as long as they live. The same is true of other corpora, already being computerized, with opportunities lost because no widely accepted conventional system exists.

Modern technology, too, can play a role in all this, impossible a few years ago. For example, W. Lenders (1971) has expressed his dissatisfaction with the "static" nature of most lexicons, suggesting "dynamic" ones instead, which never assume final form. These would exist only on magnetic tapes or discs, and be modified

constantly according to new data received. He cites as an example the "cumulative dictionary" being developed at the Institute for Communications Research and Phonetics at the University of Bonn. Here one foresees opportunities for the inclusion of a great deal more contextual material than is possible in written compendia, extremely useful in deciding the sociolinguistic status of an item or feature.

In conclusion, the proliferation of international publications and conferences would seem to make the need for a marking system both timely and acute. Obviously, whether action begins with LACUS or not, the task is one requiring the collaboration and consensus of international committees, possibly under the aegis of UNESCO. A sociolinguistic notational system, in the final analysis, would provide the scholar in Japan, reading about Paraguayan Spanish, Jamaican English, or Taiwan Chinese varieties, with a dimension of understanding beyond mere surface translations of the examples given, going little beyond the basic denotations.

As specialists in both language and linguistics, today's atmosphere of ferment and "agonizing re-appraisal" provides us with unique opportunities to achieve a new symbiosis in linguistics. This could develop from a better balance between work on field data, adequately marked and speculative activity. In this way language variation and change might be truly viewed synchronically, in both its deeper and surface manifestations.

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VI

LINGUISTIC DEVELOPMENT AND CHANGE

ON RECONSTRUCTING "PROTO-HUMAN"

Marge E. Landsberg

Haifa University-Linguistics Department

The present case for "proto-human" (U.P.S.)[1] can be summed up as follows:

(a) Man was probably walking on two legs three or four million years ago.

(b) There is enough fossilized evidence to follow his way from Africa (presumably Ethiopia), via ancient landbridges and linkages, through the Middle East, into India south of the Himalayas, into a southern extension of the Asiatic continent that is now the island of Java, and to China and the Americas.[2]

(c) Hence, from an archeological and biological (anthropological) viewpoint, what we are apparently dealing with today, is the collective evolutionary diversifications that followed the migrations of the representatives of the ancient primordial ancestral stock. Indeed, although the idea of polygenesis may be valid enough, it just so happens that evidence for it is entirely negative.[3]

(d) Since we seem to be dealing with one hominid biologically, apparently what we're looking at is also one hominid linguistically.[4] Hence, evidently the term "cognates" is a conceptual and linguistic redundancy which-caused by a delusion-is better dispensed with.

(e) Universal elements in language suggest that it was invented only once and before dispersion and subsequent diversification.

Apparently, just as both organic and behaviour patterns are achieved and maintained-namely on the basis of information that the species has gained through mutation and selection in the course of its evolution-(i.e. gene-controlled), so also more intricate mechanisms, such as those, for instance, generally subsumed under cognition.

Aware of such general or universal principles of organization, we can find a natural extension of these same principles in the mechanisms of language. Evidently, these too are biologically (species specific, species universal) determined, i.e. biogenetically transmitted from one human generation to the next, presumably via man's DNA or RNA carrying chromosomes. Clearly then, in the remote antiquity of our hominid past, the evolution of vocal learning could only take place when genetic control over motor development slackened, and the species came

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[1] Universal Proto-Speech.

[2] Landsberg: 1974b.

[3] Ashley Montagu: 1974; Birdsell: 1972; Clark: 1970; Coon: 1954, 1962; Howell: 1969; Le Gros Clark: 1955, 1971; Wescott: 1974.

[4] Stopa: 1972.

increasingly to depend on access to external (auditory) models.[5]
(f) Thus, it seems reasonable to expect that till about the rise of the Neanderthals (and performance), the entire system was transmitted genetically,[6] and hence constant (i.e. competence only). And it seems therefore also likely that linguistic change was considerably more sluggish to take effect during the upper Paleolithic and Mesolithic than during the Neolithic and historical periods.
(g) Since present day linguistic stocks (i.e. the geographical variations of human speech), apparently, are in essence then but regional dialects, resulting from the dispersion and subsequent diversification of the primordial system, evidently, general rules for dialect may apply to them.

Now then, however differing the surface manifestations of a group of dialects may appear to be, it is very probable that they share the same or a very similar system of underlying representation. Indeed, Chomsky and Halle[7] point out that it is a widely confirmed empirical fact that underlying representations are fairly resistant to historical change which, by and large, tends to involve late phonetic rules.[8] Today's regional dialects (languages of the world) then, may well still share these underlying representations. If it is indeed so that these are fairly constant, it would follow that beneath the differing surface manifestations, the same system of representation for underlying forms will be found over longer stretches of space and time than was previously assumed.[9]
(h) If that is so, then apparently, historically attested morphemes, even at what may seem extreme retro-horizons, don't have to be distorted beyond recognition.
(i) And so that is why even today all dispersed and diversified linguistic stocks still share an identical nucleus of productive morphemes (etyma), that recur as signifying elements in their expanding matrices, resulting in a PANLING network of phonetic markers representing semantic fields, such as for instance: *BHER-, *BHU-, *GER-, *KAP-, *LI-, *MAN-, *PEKW-, *PIK-, *PLEU-, *SEK-, *STA-, *TAK-, *TEM-, *WLEQ-, *WOL(W)-, etc.[10]
(j) Some of these recurring sound-patterns (motor-patterns) may belong to a system of inevitable, non-arbitrary[11] (i.e. significant) symbol manipulation, demonstrating the (bio)phonetic representations of associated semantic interpretations, constituting the internalized system of grammatical rules, or the possible biological production

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5 Nottebohm: 1972.

6 van Ginneken: 1927.

7 Chomsky and Halle: 1968.

8 Halle: 1964; Kiparsky: 1965; Postal: 1968.

9 Kazazis: 1969; Lenneberg: 1967; Sapir: 1972.

10 Hörmann: 1971; Jacobowitz: 1968; Landsberg: 1974a, 1975a.

11 Darwin: 1969; Ellenbogen: 1970; Landsberg: 1975b; Paget: 1963; Paul: 1920; Sinclair: 1971; Swadesh: 1972; Wundt: 1911.

model that generates meaningful utterances in human language.[12]
(k) If we are interested in establishing a framework of descriptive adequacy,[13] here then is at least a concrete proposal to work with. Using the data available so far, we may now also examine the possibility of building models for early hominid language.
(l) Ultimately, there are probably also some very important multidisciplinary implications, such as for instance:

(i) Historical linguistics: etymology may profit immensely by having its bases enlarged, and by thus now being able to chart at least some of its "o.o.o."s[14] out of the way.

(ii) Anthropology: enriched paleo-linguistic data may help to provide a more realistic and complete record of ethnic movements in prehistoric times.[15]

(iii) Psychology, and modern linguistic theory: in addition to there being important psychological and psycholinguistic implications -e.g., for theories of language genesis, sound-gesture correlates, phonetic symbolism, and language acquisition-there may well be others of a more immediate and practical nature, such as, for instance, the obvious usefulness of a universal inventory for language teaching methodology and translation theory.[16]

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12 Ellenbogen: 1970; Lenneberg: 1967; Meader and Muyskens: 1950.

13 Chomsky: 1965.

14 "Of obscure origin".

15 Diebold: 1960; Dyen: 1956; Swadesh: 1951, 1952a, 1959; Tovar: 1954.

16 Le Cron Foster: 1969, 1970, 1971; McNeill: 1971; Marano: 1950; Menyuk: 1971, 1972; Miron: 1961; Smith and Miller: 1971; Taylor: 1963; Wescott: 1974.

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ON THE GENERATIVITY OF NEOGRAMMARIAN LINGUISTIC CHANGE

T. L. Markey

University of Michigan

Full fathom five thy father lies;
  Of his bones are coral made:
Those are pearls that were his eyes:
  Nothing of him that doth fade,
But doth suffer a sea-change
Into something rich and strange.

The Tempest I, ii, 394

The construction of new paradigms, in the sense of Thomas Kuhn (1962), denotes epistemological revolution, and modern linguistics has been distinguished by all the fortunes and follies of revolution. During the past two decades, the focal point of theoretical inquiry has been a controversy over the relative merits of competing paradigms, structural vs. generative. Models are justified in terms of quantifiable interpretative insights afforded, for they are constituted to render the previously incomprehensible immediately intelligible. Consequently, the bulk of recent linguistic inquiry may be viewed as progressively attenuated dialectic between proponents of structuralism and advocates of generative grammar appealing to relative effectivity. Within this dialectical framework, a particular phenomenon is subjected to application of the paradigm selected and observations are rendered cognitively meaningful. The paradigm is thus supported and its efficacy thereby enhanced. Examples of studies which follow this course could be cited in legion; a concrete instance is Naro's (1971) investigation of assimilation and dissimilation wherein he seeks support for the axiom that added rules have the same form as synchronic rules.

General epistemic properties of the competing paradigms are characterized as follows. The structural paradigm is *static*, establishes a *structure*, follows a *mechanistic* line of inquiry and purports to be *idiographic*, for it claims to be *data oriented* and *descriptive*. The generative paradigm is *dynamic*, outlines *processes*, follows a *teleological* line of inquiry and purports to be *nomothetic*, for it claims to be *theory oriented* and *analytic*. One must not, of course, impute mutual exclusivity to this assignment of values. Many general linguists, such as Buyssens (1965), have employed structuralism with an eye to teleology and analysis; while, conversely, highly theoretical generativists, such as Kenstowicz and Kisseberth (1971), have drawn nomothetic conclusions from data oriented descriptions. Indeed, there is fundamental propositional convergence of the models, for both necessarily presuppose that language exhibits the characteristics of a system composed of isolatable variables and, more significantly, invariables. Both necessarily claim similarity to reality to ensure operational significance of their logical constructs.

Both paradigms were initially applied solely to the synchronic sphere: structuralism to synchronic phonology and generative grammar to synchronic syntax. This restriction may have resulted from conscious avoidance of the "difficulties" imposed by the time factor and inchoate realization that these "difficulties" might be unresolvable in terms of the respective paradigms.

Saussure (1959:79) noted that there is an inner duality imposed on all sciences concerned with values when a time factor is introduced, i.e. the synchrony (static) vs. diachrony (dynamic) dichotomy. This dichotomy, which Saussure endowed with the positivist concepts of Émile Durkheim (1858-1917), was establish -ed as dogma. Synchronic linguistics studies terms that build a system in the *conscience collective*, while diachronic linguistics does not: the material of synchronic linguistics are *représentations collectives*.

In French anthropology and sociology, the heirs of Saussurean structuralism and Comtean positivism, this dogma was pressed to absurd extremes by Lucien Lévy-Bruhl (1857-1939), while its abandonment by Claude Lévi-Strauss subsequently led to instructive findings. Throughout the 1950's prominent American structuralists, such as Bernard Bloch, George Trager, Martin Joos and Einar Haugen, remained primarily interested in synchrony and divorced themselves from philological tradition in which they, as well as the apotheosis of their endeavors, Leonard Bloomfield, had been trained. Generativists, too, eschewed diachrony until it was made palatable a decade ago by Paul Kiparsky, though Anttila (1973) wonders whether or not generative historical linguistics ever existed.

Categorical distinction between areas of application, whereby structuralism was initially and primarily equated with synchronic phonology and generative grammar with synchronic syntax, is due to putative restrictions of the models. The sounds of a language were deemed better seen as items to be provided with a discrete, static structural arrangement, while its infinitely divergent syntactic structures were deemed better viewed as the result of non-static transformational processes. However, as Piaget (1970) astutely contends, both models necessarily incorporate the notions of structure (item-arrangement) and transformation (item-process).

The two fundamentally "difficult" processes introduced by the time factor are the typologically stochastic processes of analogy and its converse, differentiation. These processes conspire to yield dialectal split, yet simultaneously preserve a modicum of similarity so that mutual intelligibility permitting comparability persists. Analogy and differentiation function as both conservative and innovative forces. Analogy can either operate to preserve a paradigm or alter its identity, as can diff -erentiation. Examples are well documented in the literature. Akin to dialectal split are pidginization and creolization. Analogy and differentiation are probably prime movers behind such contact situation processes. Indeed, within the web of relation -ships which obtain among grammatical categories, speakers' foci, contact situation(s) and lexical items located on contin-

ua, complex rules termed concessional probably govern the possible range of analogical and/or differential reformations which ultimately engender pidgins and creoles, cf. Leed 1970-71. Given two contact languages (X) and (Y), (X) lacks tense markers, but its aspect markers are realized as functionally analogous to tense markers in (Y), which lacks aspect markers, and reified as tense markers in the resulting pidgin. Analogy and differentiation also impinge upon restricted instances of dialect borrowing. Cases in point are gaps within self-optimizing grammatical subsystems such as modal auxiliaries. Old Swedish borrowed MLG *mōten* > *māsta* to fill a gap in its modal system. The borrowed modal was grammatically and semantically analogous to native OSw. *mā/mātte*, but formally distinct, cf. Winter 1969. Similarly, analogy and differentiation provide the entelechy of morphological innovation by filling gaps with potential forms; *frigid* : *frigid -ness* :: *rigid* : *X*. Diachronically, analogy and differentiation impose dialectal split, pidginization, creolization, and restricted instances of dialect borrowing and morphological innovation, cf. Meillet1966. Failure to account for analogy and differentiation necessarily results in failure to account for these diachronic processes.

Both structural and generative grammar have failed to account for analogy and differentiation, and analogy was long excluded from classical generative theory. The theory's conceptual rigidity as a formal interpretative device prevented it from accounting for stochastic processes. Moreover, strict adherence to rigorous formalism permitted the postulation that synchrony recapitulated diachrony. Counter-examples to this postulate entailed well-known discussions of intermediary rule insertion, analogy as simplification, opacity and paradigmatic regularity and the abstractness of phonology itself, e.g. Kiparsky 1968a, 1968b, 1971, King 1969, 1972, 1973, Miranda 1971, Schindler 1974. Analogy was officially condoned by papal ukase with the announcement of a generative morphology by Halle (1972) in a report which remains an embarassment: linguists schooled in the principles of Hermann Paul (1960) have long re -cognized the efficacy of the proportional model in interpreting morphological innovation, a model ingenuously propounded by Halle as novel. It was not merely conceptual rigidity coupled with adherence to strictly regulated rule mechanisms (analogy = simplification) and the synchrony-diachrony postulate which serv -ed to exclude analogy, but also exclusion of graduality as a possible typology of change, e.g. Postal 1968, cf. Markey 1973. It has long been accepted that analogy and differentiation can be characterized as either gradual or abrupt.

Generative historical linguistics fails to describe and /or account for precisely the same aspects of change as the Neogrammarian paradigm in its strictest adherence to the dogma of *Ausnahmslosigkeit*. As both theories formulate and apply absolute rules (laws) abstracted to describe general tendencies, both are explanatorily inadequate with respect to variant periodicity of change(s), analogical distortion of regular application of rules, renewal occasioned by random processes (e.g. homonymy) and op-

eration of secondary changes which induce reversion to superficially original states. Despite differences in terminology and notational conventions, both theories fail to account for analogy and differentiation. Note that, while generative historical linguistics sought to exclude or reinterpret analogy, the Neogrammarians, though they also regarded analogy as aberrant, never questioned its legitimacy as a process.

Elsewhere (1974), with reference to Brugmann (1876), we attempted to dispel some of the current misconceptions, proliferated by Jankowsky (1972), centered around the Neogrammarians, cf. Pulgram 1955, Putschke 1969. Not all Neogrammarians were absolutists in promulgating and applying the *Ausnahmslosigkeit*-principle, just as not all generative historical linguists are absolutist in their denial of analogy. However, their paradigms are conceptually identical in that both derive forms by mechanism of regularized rule relations. As is the case with generative historical linguistics, Neogrammarian principles were attacked less than a decade after codification, but the attack persisted more than half a century, e.g. Debrunner 1933, Hermann 1931, Löwe 1891, Schuchardt 1885, Wechssler 1900. Lack of definitive resolution was presumably due to the impenetrable nature of analogy, which Scherer(1878) had scrupulously defined as being functionally distinct from *Formübertragung* and differentiation. Parallel to and in collusion with the anthropologists of the day, such as Andrew Lang (1844-1912), E. B. Tylor (1832-1917), A. R. Radcliffe-Brown (1881-1955) and B. K. Malinowski (1884-1942), the Neogrammarians were functionalists: reciprocal influence between anthropology and linguistics was, as we have seen, to be repeated after the advent of structuralism. Linguistic history has now repeated itself; for, with the attack on generative historical linguistics and dialectology, the analogy debate has become topical once again, e.g. Anttila 1974, Best 1973, Fónagy 1957, Höffding 1967.

Perennial asseverations to the contrary, analogy is *sui generis* stochastic and cannot, therefore, itself be interpreted in terms of laws, as Kuryłowicz (1945-49, 1958) has prescribed, nor in terms of statistics, as Herman (1951-52) and Mańczak (1957-58, 1963) have attempted. Kuryłowicz's laws are logical contradictions: individually defined as discrete, they permit arbitrary concatenation (1-4-3, 3-1-4, etc.).

If we are to come to grips with the intrinsic mysteries of change, we must penetrate the functional qualifications of analogy with, of course, reference to the societal and psychological factors which surround and actuate it.

Anttila (1974), together with Henning Andersen, has convincingly defined analogy cognitively as an abductive process and thereby placed the discussion in the arena of perceptual behavior. The fundamental linguistic datum of analogy is allomorphy with definable semantic associations and paradigmatic congruity, e.g. Gmc. *holt-a* nom. sing. : *hult-is* gen. sing. > *holt-a* : *holt-is*, though we could equally well expect *hult-a* : *hult-is* with reversion to the original state prior to *a*-umlaut.

The formal propositional issue at stake in any further analysis is an ultimate predictability requirement; we need a gen-

eral calculus to determine the probability of analogical influence. Criteria for such a calculus are functional proximity/distance (e.g. an objective accussative and dative), privileges of sequential occurrence (e.g. phonotactics), degrees of paradigmatic congruity (e.g. members of paradigms with and without medial affrication before front vowels) and semantic association (e.g. agents for inanimate vs. animate actors, German *durch/von*), as well as realization of the abductive actuation of analogy measured in terms of entropy/redundancy (similarity/identity). The effectivity of such a calculus is imperiled by the stochastic nature of analogy unless a specific function is ascribed. An apparently primary function of analogy is effectuation of paradigmatic stability, but recognition of purpose and a general calculus based on the above criteria may not permit us to fulfil the ultimate predictability requirement: the directionality of analogical reformation may eternally elude prediction.

We conclude with a well documented example. Some Northern Swedish and Fenno-Swedish dialects have progressive medial affrication of *k/g* before front vowels: *vägen* > *vädjen* > *vädẑen*. The nominal paradigm in Standard Swedish, which lacks affrication, is:

sing.	indef.	definite	pl. indef.	definite
I	*väg*	*vägen*	*vägar*	*vägarna*

and regular application of the affrication rule yields:

II	*väg*	*vädjen*	*vägar*	*vägarna*

while some Northern and Fenno-Swedish dialects have:

III	*väg*	*vädjen*	*vädjar*	*vädjarna*

and yet other Fenno-Swedish dialects have:

IV	*vädj*	*vädjen*	*vädjen*	*vädjarna*

Assuming that the affrication rule was once in general force, (II) represents the regular *lautgesetzlich* paradigm, while (I) and (IV) represent opposite extremes of analogical levelling with (III) as an intermediary step toward (IV). Given (II), one could predict (I) on the basis of statistical probability, but not (IV). Given (III), one might predict (IV), but not (I). The directionality of the levelling of the allomorphs (*väg/vädj*) defies prediction.

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ANALOGY: MENTAL PATTERN OR MENTAL PROCEDURE?

Thomas McClellan

Texas A&M University

In his paper Phonetic analogy and conceptual analogy (1971) Theo Vennemann cites the case of an individual who pronounced the name of a well-known linguist /nowm/. After he was corrected by members of the "in group," he was heard to say /howəm/ for home, /rowəm/ for roam, and then /nowən/ for known. At this point he stopped extending his new pronunciation of the name /nowəm/ to other words.

Vennemann, who rejects the "pre-transformational" proportional model for analogic change because it "has failed to create a coherent theory," accounts for the phenomenon as follows: the speaker, let us call him Noaman, introduces a sporadic rule (A1) into his grammar, generalizes it (A2) to produce /howəm/ and /rowəm/, generalizes it again (A3) to produce /nowən/, and replaces it by relexicalization (A4).

(A 1) $\emptyset \rightarrow$ ə / [now_m]Name

2) $\emptyset \rightarrow$ ə / ow_m #

3) $\emptyset \rightarrow$ ə / ow_N #

4) /nowm/ $\rightarrow$ /nowəm/

That descriptions such as (A) have provided valuable insights into language change and dialect differences is undeniable. At the same time, Vennemann's application of such descriptive tools to an individual's behavior seems a poor choice. It involves him in a difficult psychological model, including Noaman's "innate knowledge of what constitutes a phonetic generalization" as well as his ability to consciously "surpress" the rule created and generalized by this innate knowledge. Also, since transformational-generative theory provides no model of performance to account for what Joos called "clocks" or for what Hockett has called "dequotation," description (A) implies that Noaman may say sentence (x) but not sentence (y):

(x) My dearest darling, at last we're /əlowən/.

(y) *This has been /nowən/ -- I mean /nown/ -- since Chomsky published Aspects.

Finally, description (A) makes the difficult-to-support claim that Noaman did not respond to the similar phonetic shapes of his "old" pronunciations of /nowm/ and /nown/ in their entirety; rather that he regeneralized a rule designed to accomodate only the last three phonemes

of any lexical item (A2 becomes A3).

In short, description (A) gives us an overcomplicated psychology and an oversimplified, counter-experiential view of how real people behave.

The proportional frame model could be used to account for Noaman's performance as an analogical extension of his new pronunciation of Noam to three other words.

(B 1) /nowm/ : /nowəm/ :: /howm/ : X X = /howəm/

2) :: /rowm/ : X X = /rowəm/

3) :: /nown/ : X X = /nowən/

That real people analogize both in learning and in using language is fairly well attested. The high incidence of clang associations (phonologically similar responses) to nonsense words in word association tests indicates that one possible reaction to the phonologically unfamiliar is to relate it to the phonologically familiar. Jesperen's (1922) example of the Danish boy who justified his creation of a strong preterite for the verb nikker (nod) with the phrase stikker stak, nikker nak is a commonplace, and Sturtevant (1947) cites a similar justification by an American three-year-old, sing sang, swing swang. Most of us could add at least one example to the list; dove, for instance, as the preterite for dive (most probably on the analogy of drive, drove) appears to be fighting its way into American English in defiance of both forces Vennemann proposes to account for analogic change -- phonological and conceptual regularity.

Description (B), then, stretches our credulity less than (A). At the same time, it lacks what description (A) has in abundance -- predictive value. As Kuryłowicz (1949) puts it, the proportional model gives us "a prepared path -- gutters, sewers, conduits" for water to follow "when it rains," without indicating the probability of rain, much less whether there will be a shower or a downpour.

One step we may take in attempting to enhance the predictive power of the traditional model for analogic change is to discover how individual people analogize. Unfortunately, the mental process sketched by the proportional frame is a complex one made more complex by paraliguistic factors. This I can vouch for from experience.

Several years ago I attended an NDEA workshop at Texas Southern University, a predominantly "black" school (the term was not then in favor) in a predominantly "black" neighborhood. In this context I was a member of a racial minority. While walking near the campus one afternoon I glanced up to see a street sign -- TUAM. For a moment I could not perceive the string of letters as a word, then I saw the word GUAM as if superimposed on the sign, after which a couple of scenes from war movies flitted through my mind -- the flag being raised on Iwo Jima and Japanese planes bombing a fleet. I then muttered to myself /gwam twam/ and went on, noting that the place had a puzzling oriental feel to it. When my pronunciation was corrected to /tuəm/ by a native Houstonian, I was again puzzled by a feeling about the place, this time that it was in

some way related to American Indians; and again an image presented itself to me, this time of a dark, forboding edge of woodland.

Guam and Tuam share a number of properties in addition to visual similarity: both are nouns, proper nouns, place names; both, in terms of my context, were associated with non-white races and racial conflict. When I considered the analogy along these lines, the American Indian association became explicable. The place name Manitoba, according to an etymology I learned in grade school, derives from an Indian word for spirit, /maenətu/. That I had seen motion pictures involving conflicts between Indians and white settlers goes without saying. The original spelling analogy could be described by the proportional frame model thus:

(C) Guam : /gwam/ :: Tuam : X X = /twam/

But this description does not capture the most interesting facet of the event -- that the association pattern tended to replicate all its constituents after one constituent was changed.

(D) Guam_____Tuam Tuam.....Manitoba

/gwam/------/twam/ =========/tuəm/------/maenətu/

Japanese.....Blacks Blacks_____Indians

The relative strength of the lines indicates what I believe to be the primary [___], secondary [---], and tertiary [...] forces of similarity in the original analogy and in the replicated pattern. The visual similarity between the place names dominates the first; the contextual similarity between my notion of myself as a white trespasser in black territory and the situation of settlers in Indian territory dominates the second.

A similar, though far more complex, illustration of carry-over from one association pattern to another is found in Freud's Psychopathology of everyday life (1901). The author tells of conversing with a companion about the customs of the Turks in Bosnia and Herzegovina as they were driving toward the latter city. He mentioned that a colleague had told him the Turks expressed their resignation to a doctor's diagnosis that nothing could be done for a patient with such phrases as "Herr, what is there to be said? If he could be saved, I know you would have saved him." This called to his mind a similar expression his colleague had reported to illustrate the Turks' attitude toward sexual pleasure: "Herr, you must know that if that comes to an end, life is of no value." But, loath to discuss sex with a stranger, he did not mention it. This phrase was also linked to a memory of news he had received while in Trafoi, that one of his patients had killed himself because of an incurable sexual disorder. He then changed the topic of conversation, asking his companion if he had ever been to Orvieto and seen the frescoes there of The Four Last Things (death, judgment, heaven, and hell) by . . . . At this point he could not recall the name of the artist (Signorelli), but thought of a similar

name of another Italian artist, Botticelli, and a not-so-similar name of still another, Boltraffio. Freud diagrams the interrelationship of these items as follows:

```
(E)  [Signor]elli          (Bo)tticelli      (Bo)ltraffio
      |  ↓                  ↑                  ↑
      |  [Her]zegovina and (Bo)snia            Trafoi
      ↓
     [Herr,] what is there to be said?
       ↘
        Death and sexuality
                  ↘
                   Repressed thoughts
```

Though Freud shifted the conversation from what a Turk said about death in Herzegovina to what an Italian painted about Last Things in Orvieto, the DO NOT MENTION tag he had attached to Herr (if that comes to an end ...) became affixed to the Signor- of Signorelli.

This tendency toward replication of association patterns offers us a third possible hypothesis concerning Noaman's analogical extension of a revised pronunciation: that he acquired his original pronunciation on the basis of a spelling paradigm for words with oa between two consonant symbols and that the correction to /nowəm/ gave birth via replication to another (short-lived) paradigm for "long o" words of one syllable.

```
(F)  boat
     coat
     loan                          /nowən/
     moan                          /howəm/
     roam                          /rowəm/
      :                               :
      :                             _ow_
     _oa_ -- Noam          Noam--- _oa_

    /_ow_/--/nowm/======/nowəm/--/_owə_/
```

One implication of this description seems dubious -- that Noaman might say (in contexts where the "in group" pronunciation would be important) /kowət/ for coat, /bowəl/ for bowl, etc. Even so, I believe (F) offers a more credible psychology than (A) and more predictive value than (B). As for what actually went on inside Noaman's head, no one save Noaman himself knows.

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SYNCHRONIC ALTERNATION AND DIACHRONIC CHANGE

Seok Choong Song
Michigan State University

In a synchronic description, alternate shapes of a linguistic form are accounted for in various ways. A linguist may simply list them in case of suppletion. If the alternation is conditioned phonologically, a systematic rule can be formulated to derive one form from the other, which is considered a base or an underlying form. In some cases, both alternants are derived from a more abstract representation which never surfaces but nevertheless captures an underlying regularity of a language. When the alternation is morphologically conditioned, a more complicated rule utilizing some non-phonological feature is in order.

In all these cases, descriptions simply show how one form is related to the other and, at best, point to a process by means of which such an alternation might have come into existence, as in the case of an assimilation. Although it is undeniable that certain synchronic alternations may capture or dovetail with a historical change of a language, there is a danger in equating a synchronic rule with a historical development. Linguistic change is extremely complex, and diverse operations are at work pulling and tugging in different directions, but at the same time maintaining a delicate equilibrium in a system. Needless to say, a historical change and a synchronic alternation are intricately interrelated, influencing and influenced by each other. "Without variation" as Anttila (1972) aptly puts it, "one could not understand change, and without change, one would not understand synchronic variations." (p. 190) But what appear to be phonologically related alternants of a single morpheme may on occasion come into existence through a historical accident called a convergence rather than a sound change.

The main purpose of this paper is to show that alternate shapes of the nominalizer in Korean are historically unrelated despite the fact that the relation between their phonological shapes can be stated by a natural phonological process. Since I have had to depend on the scanty evidence available, and on inadequate studies of the historical development of the Korean language, the history itself is largely a hypothesis. But this hypothesis not only casts a new light on the historical development of the nominalizer, but also provides a strong support for my earlier thesis on negation in Korean.

In Song (1967), I argued against the traditional claim that Korean has two types of negative sentences, supposedly in free variation, corresponding to a single affirmative counterpart. The crux of

my argument was contingent upon the identification of two shapes ki and ci as allomorphs of a single morpheme, i.e., the nominalizer. Consider the following sentences.

(1) John i coffee lul masin-ta[1]
NOM Acc drink-Dec
'John drinks coffee.'

(2) John i coffee lul ani masin-ta
Neg
'John doesn't drink coffee.'

(3) John i coffee lul masi ci (nun) ani han-ta
Nom do-Dec
'It is not the case that John drinks coffee.'

Against the previous accounts that claim that both (2) and (3) are negative counterparts of an affirmative sentence (1), I matched (3) with (4) and demonstrated fairly conclusively that there is a one-to-one relationship between affirmative and negative sentences in Korean as in many other languages of the world. Now compare (4) with its negative partner (3).

(4) John i coffee lul masi ki nun han-ta
Nom
'It is the case that John drinks coffee.'

The only difference between the two sentences is that in the affirmative sentence (4), the shape of nominalizer is ki rather than ci, which we find in the negative sentence (3). The following rule was given to account for the alternation between ki and ci.

(5) ki ⟶ ci / V __ Neg + ha

This rule is well-motivated syntactically, and both historical change and dialectal variation seem to provide a strong support.

Speakers of several dialects in Korea today palatalize the initial k before a high front vowel.

Standard Speech Form (Seoul dialect)	Dialectal Variants	
kil	cil	'road'
kilum	cilum	'oil'
kichim	cichim	'cough'
kim	cim	'seaweed'
kil-ta	cil-ta	'is long'
kiph-ta	ciph-ta	'is deep'

Historically, the present day ki and ci are believed to have a common ancestor ti. It is well attested that in Middle Korean (1000 - 1600)

the form ti was used as a nominalizer as following examples show.

(6) kacyeka ti elyeul ssay
carry Nom difficult that
'That it is difficult to carry'

(7) kelsik-ha ti elyep ko
food-beg Nom difficult and
'(It is) difficult to beg food and . . .'

(8) mek ti tyoha-mye ssu ti tyohan kesila
eat Nom good-and use Nom good thing (it) is
'(It) is good to eat and good to use.'

Sung-Nyong Lee (1961) remarks that "it should be noted that the form ti is used like the noun-formative ki. Thus mekti [in sentence (8)] has the same function as mekki . . .." (p.263) Some Korean scholars seem to believe that the modern ki is a direct descendant of the form ti of Middle Korean.[2] It is certainly tempting to draw a conclusion that both ki and ci go back to the common ancestor ti. Furthermore, the phenomenon of k becoming palatalized before a high front vowel is a natural process found in many languages and thus the rule (5) has an abundant support from the point of view of 'Universal Grammar.'

There are, however, a few questions remaining still unanswered. Historically, we find no other examples that justify the split of ti into ki and ci. The historical change of ti > ci and of ki > ci in some dialects are well attested but ti > ki seems to be an isolated instance. Furthermore, the assumption that ti changed to ki in an affirmative sentence while it changed to ci in a negative sentence will remain a riddle that defies a solution. Synchronically, the nominalizer ki becomes ci in one particular type of negative sentence in all dialects, thus, the alternation is grammatically conditioned. In Seoul dialect, where ki~ci alternation in morpheme initial position does not take place, the rule (5) is required. Also in all those dialects which show ki~ci alternation, the grammatical morpheme ki is not affected; therefore, the same rule (5) is required. Although the alternation of the nominalizer ki~ci is synchronically well-motivated, the rule (5) becomes suspicious if interpreted as a case of grammatically conditioned palatalization rule.

If we look at the historical data a little more closely and postulate a slightly different hypothesis about the development of the nominalizer, both diachronic and synchronic problems seem to simply disappear. Sung-Nyong Lee points out ki is also used in Middle Korean as a verbal suffix to derive gerundive nominals. This treatment, of course, is typical of the structuralist approach and all the previous descriptions of the nominalizer ki in present day Korean are essentially of the same nature. It was considered as a kind of a verbal affix added to verb stems to derive nominals. I treated it as a nominalizer in 1967 and more recent descriptions treat it as a complementizer. I suspect that the ki might have also been used as such in

Middle Korean. My suspicion is confirmed by a historical study of Korean by Ki-Moon Lee (1972).

(9) posi-ha ki lal culkye
alms-give Nom Acc delight
'. . . delights in giving as a temple offering'

(10) kul su ki wa kal ssu ki wa payhoni
letter write Nom and sword use Nom and learn-and
'(One) learns how to write and use sword and . . .'

Ki-Moon Lee, however, promptly points out after giving these examples that such a usage is rather rare in Middle Korean.

When we examine negative sentences, we seem to find abundant instances of ti used in a fashion parallel to the nominalizer ci in modern Korean. Compare the following examples with the negative sentence (3).

(11) i ptu tul nis ti malasyosye
this meaning Acc forget Nom Neg-Imp
'Do not forget this meaning (or intention).'

(12) huli ti ani hamye say ti ani halssay
muddy Nom Neg be-and leak Nom Neg do that (it) is
'That it is neither muddy nor leaking . . .'

It is clear that these sentences match morpheme by morpheme with their modern counterparts except for a few archaisms in spelling and grammar. The historical change of t>c before a high front vowel or a semi-vowel (i and y) is also a well established fact by now.

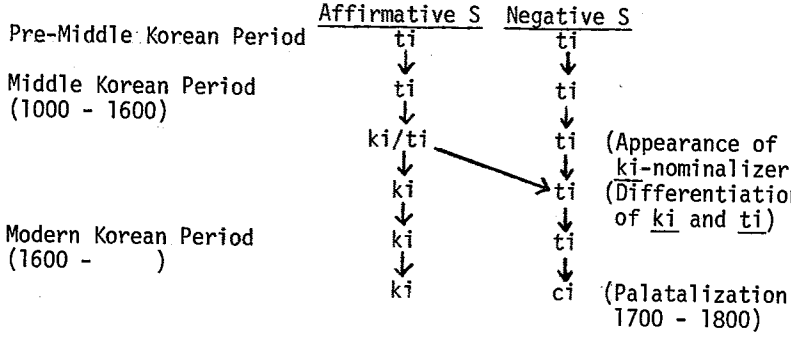
Let us summarize what has been said so far about the nominalizer in the Middle Korean period to get a clearer picture of that stage in its development.

(i) The shape of the nominalizer was ti.
(ii) The nominalizer was used both in affirmative and negative sentences.
(iii) Another nominalizer ki began to appear on the scene in an affirmative sentence.

At the moment I have no idea where ki comes from and how it has come to be used along with ti in affirmative sentences. It may not be a totally idle speculation to conjecture that it originally was a verbal suffix to derive nominals and came to be used as a nominalizer or complementizer by extention. If Ki-Moon Lee's observation is correct, which I believe it is, the ki-nominalizer began appearing in the Middle Korean period. It is conceivable that, at least for some time in this period, both ki and ti must have been used as nominalizers but the former seems to have been restricted to affirmative sentences while the latter without restrictions. It should be emphasized here that

what I am suggesting is that ki is not a variant shape of ti at this stage, but a totally unrelated form moving into the semantic domain of ti or rather its domain of grammatical function.

The fate of doublets or duplicated forms is a familiar story. One of them either drives the other out of existence or push it out of its original semantic area into a new one. When Korea and Japan fell under the cultural domination of China early in their history, numerous Chinese words made their way into the vocabulary of these languages. A large stock of native vocabulary lost the battle and became extinct, and even today more than 50% of the total vocabulary of those two languages is of Chinese origin. Where the duplicated forms survived, differentiation took place and no real synonyms seem to exist. The generalization about the vicissitude of duplicate forms of lexical elements probably also apply to grammatical elements. Since the semantic content of a grammatical morpheme is hard to define, it would be simpler to talk about in functional terms. In the case of the Korean nominalizer in question, not even a functional shift took place, but simply a rearrangement in terms of distribution. The form ki began to replace the original ti in affirmative sentences and the occurrence of the form ti came to be restricted to negative sentences, creating a situation where ki and ti can peacefully coexist in a complementary distribution. When the ki, an independent and separate morpheme unrelated to ti, completely ousted its rival form in affirmative sentences, the merger of two morphemes must have taken place. Exactly when this has happened is still uncertain and subject to further study. The rest of the history of the nominalizer is straightforward. Although the relative chronology of the morphological merger of ki and ti, and the palatalization of ti to ci still has to be determined, the sound change of ti to ci eventually produced an alternation of ki and ci in the nominalizer. The historical process of palatalization in Korean has been estimated by some scholars to have begun even before the 17th century in some dialects. This process gradually swept the entire language, except for the northwestern dialect, eliminating one alternant of the nominalizer ti and replacing it with the new form ci. If the palatalization preceded the merger of the two nominalizers, the result will remain the same and my hypothesis will not be affected. We can schematically represent the development of the nominalizer as follows.[3]



I have taken it for granted that the ti's in both affirmative and negative sentences are the instances of one and the same morpheme without any demonstration of their identity. There seem to be some indications that more than one morpheme is phonetically realized as ti in Middle Korean, just as the form ci in Modern Korean must be considered as the homophonous realization of at least four different morphemes. The existence of (a) homophonous morpheme(s) will affect the historical hypothesis presented here very little, and I will continue to assume their identity for the simple reason that any other account will merely complicate the description unnecessarily. The real question is how to distinguish among homophonous morphemes, if any, but not the existence of the nominalizer ti which occurs in both affirmative and negative sentences at this stage.

If my hypothesis reflects any degree of verisimilitude of an actual historical development, then it can provide us with answers to questions raised earlier. Despite the deceptive resemblance of the rule (5) to the k-palatalization rule for some dialects, the two alternants of the nominalizer in Modern Korean come from two different, historically distinct sources. It can also provide an explanation of how a specification of a grammatical conditioning is necessary for a synchronic account of alternant forms. Finally, it will dissipate any doubt about credibility of the existence of the synchronic rule like (5), thus providing a historical justification for my Neg-hypothesis. The rule itself, however, has to be revised to eliminate its misleading appearance of being a grammatically conditioned phonological rule. I tentatively propose the following form of the rule to replace (5).

(13) $\text{Nom} \rightarrow \left\{ \begin{array}{l} \text{ci/} \\ \text{ki} \end{array} \right. \;]_{S-}]_{NP} \; \text{Neg+ha}$

Condition: NP and ha must be immediately dominated by the node VP

Although I am primarily concerned with a historical hypothesis of the development of the nominalizer in Korean, the general implication of the study of this sort is quite obvious. We cannot understand the totality of linguistic structure unless we look at it from a diachronic perspective as well as on a synchronic plane. Let me conclude this paper by quoting Emmon Bach (1974), who succinctly summarizes the situation in a nutshell.

> There was a period when linguistics was historical linguistics. A slogan of that period was: You can't understand a language without knowing its history. There came another period with the slogan: You can't understand how languages change without knowing what they are. Now the truth is that we can't understand language without working on both problems, moving back and forth, testing hypotheses about one aspect of the totality of linguistic facts by seeing how well it fits with hypotheses about the other aspect (and still other aspects). (pp. 167-68)

Without my Neg-hypothesis which claimed the identity of ki and ci on a purely synchronic grounds, no historical hypothesis of the nominalizer would have been possible. The historical hypothesis I present here, on the other hand, seems to offer a plausible explanation for intriguing questions related to the synchronic alternation of the form.

NOTES

I am indebted to David Lockwood and Rachel Costa for reading an earlier draft of this paper and making suggestions for improvement of the content as well as style.

[1]I have used the Yale Romanization in transcribing Korean with very little modification. The following abbreviations are used in glossing grammatical morphemes.

NOM: Nominative	Nom: Nominalizer
Acc: Accusative	Neg: Negative Particle
Dec: Declarative	Imp: Imperative

[2]Fred Lukoff first pointed out to me that ki and ti have a common ancestor ti in Middle Korean. (Personal communication.) In dictionaries I have consulted, the following notations occur after the entry ti. (ti Mid. K.> ki in Mod. K.) It is not obvious how to interpret the arrowhead, but the lexicographers seem to imply that the modern form ki is a direct descendant of the earlier form ti.

[3]It is quite conceivable that the actual history of the nominalizer can be much more complex than what I have presented here when we take into account dialectal variations, which I have ignored here entirely. A complete history must await a future investigation of historical dialectology.

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VII

LANGUAGES IN CONTACT

TITLES TRANSLATED UNIDIOMATICALLY

Saul Levin
State University of New York at Binghamton

Among bilinguals, those who translate books have a particularly important role. The texts that they or their employers select are relatively few and of more than ephemeral and purely national interest, unlike the great mass of manifestations of language, written and oral. The translator understands the words not just passively, as an ordinary reader does, but must consciously, deliberately restate the meaning in another language. Since he is not the author, he is liable at times to misstate, like anyone attempting to paraphrase what he has not quite grasped. Furthermore, hardly any translator is a perfect master of both languages, not separately but simultaneously, so as to come up with a neat and accurate equivalent without fail.

Titles are surprisingly prone to mistreatment. Although it would seem natural for a translator to give somewhat more thought to the title, because of its prominence, than to a phrase of the same length in the body of the text, still one can point to many conspicuous lapses. The ones that I mention are of course those which have jarred *my* Sprachgefühl.

(a) *The Brothers Karamazov*, etc.

Consider, first, *The Brothers Karamazov*, which renders **Братья Карамазовы.** Pure English differs from Russian in that the family name precedes *brothers*. We say *the Dean brothers, the Wright brothers, the James brothers, the Marx brothers, the Barrymore brothers, the Kennedy brothers*, not **the brothers Dean*, etc. Along with posterior position, *brothers* gets enclitic or weakened stress; we would hardly say *the Dean br'others* with full stress except to distinguish them from the Dean sisters.[1] The stress-pattern holds no matter how long the family name may be: *the Montgomery brothers, the MacNamara brothers*. Even if the family name is quite foreign, as in *the Colombo brothers*, or -- to take a Russian example -- *the Ulianov brothers*, the English rule of syntax prevails as long as we speak English, with the English word *brothers* and the English article.

How then, could Constance Garnett, the translator of **Братья Карамазовы**, do the opposite? Or has our language changed in this

1. We may of course say *Brother Dean*, but with reduced stress on *Brother* unlike the full stress on *Brothers* in *The Brothers Karamazov*. Semantically as well as accentually *Brother Dean* is different from *the brothers so-and-so*. *Brother Dean* signifies, whether sincerely or just formally, that he is '*my* brother' -- i.e., that the speaker loves him like a brother. There is no such implication in *the Dean brothers*, nor in *The Brothers Karamazov;* they are brothers to each other, but to the speaker they are nothing.

regard since her translation came out in 1912? The question eludes a precise philological solution. Henry Sweet wrote in 1891: ". . . such combinations as *the Misses Smith, the brothers Smith* now sound pedantic."[2] He implied that *the brothers Smith* (like *the Misses Smith)* had formerly been regular; but his interest was focused upon which noun is pluralized, the surname or the title, not upon the order of the two terms. Otto Jespersen cited "the brothers Cogglesby" from Meredith's novel, *The Egoist,* and "the two brothers Crawley" from *Vanity Fair,* in contrast to "the Atkinson brothers," "the Dodson sisters," "the Tulliver children," etc.[3]

A pedantic expression is one drawn from a learned source but too remote from ordinary, sensible people. It offends their ear as an untimely display of scholarship, as though no simple, everyday expression were adequate. So far as I can make out, before Garnett *the brothers so-and-so* was associated chiefly with title-pages in a foreign setting; so she was adhering to a sort of tradition. "The Brothers Grimm" had long been given as the authors of many English editions of the *Kinder- und Hausmärchen,* going back at least to 1853.[4] Of course it is a literal rendering of *die Brüder Grimm* or *die Gebrüder Grimm.* It prompted a humorous pseudonym, the Brothers Grinn (with a double -nn!), for E. L. L. Blanchard and T. L. Greenwood, who wrote musical plays based on old fables for the theaters of Glasgow and London between 1858 and 1879.[5] Even if they were forgotten by the time Garnett made her translation of Dostoyevsky, there was a trickle of similar book-titles. *Les deux frères,* by Erckmann and Chatrian, in 1873 was followed immediately by a version entitled *The Brothers Rantzau: A Story of the Vosges.*[6] Either the anonymous translator or the publishers -- Sampson, Lors and Co., of London -- did not see fit to render the French literally 'The Two Brothers' but came up with something more picturesque and un-English.[7] The summer number of *The Studio* in 1907 was *The Brothers Maris (James -- Matthew -- William)* by D. Croal Thomson -- an English author but a Dutch family of painters; their Christian names, however, were Anglicized.[8]

2. *A New English Grammar, Logical and Historical* (Oxford: Clarendon Press, 1891), pp. 319-320.
3. *A Modern English Grammar on Historical Principles,* II (Copenhagen: Einar Munksgaard, 1948; first publ. in 1913), 20-21, 25.
4. British Museum, *General Catalogue of Printed Books,* 92, pp. 612-625. *The National Union Catalog: Pre-1956 Imprints,* 219 (Mansell, 1972), pp. 93-136, cites just one ed. with the more normal English order (p. 101): "Fairy tales from the collection of Grimm brothers. Translated from the German by Lucy Crane. Chicago and New York, Rand, McNally & Co., [188-?]." A prior publisher of Crane's version entitled it simply "Grimm's fairy tales" (New York: Allison [186-?]), which is indeed the form best known.
5. *National Union Catalog,* 78, p. 369.
6. British Museum (above, note 4), 68, p. 27.
7. On the contrary, *Les frères Zemganno* by Edmond de Goncourt (Paris: G. Charpentier, 1879) was translated as *The Zemganno Brothers* in the same year; *National Union Catalog* (above, note 4), 205, pp. 494, 508.
8. In the text Thomson alternated between "the Brothers Maris" and

Most to the point, perhaps, was a firm of wood-engravers in London called the Brothers Dalziel from 1840 to 1893, later Dalziel & Co., who illustrated many books and magazines. George and Edward Dalziel wrote a volume of reminiscences entitled *The Brothers Dalziel*, which was published in 1901 (London: Methuen) -- close to Garnett's time.

Such were the English precedents for *The Brothers Karamazov*. Garnett's subservience to the Russian word-order in the English title may have been abetted by the French versions that were already famous, called *Les frères Karamazov*.[9] *The Brothers Karamazov* is indeed a more exact calque upon the French or German than upon the Russian, which lacks the article and pluralizes the family name. The effect, if not the intention, of the odd order is to set the Karamazovs in a far-off land where no English is spoken, and so to prepare us for strange goings-on.

It superseded the more idiomatic title given by Isabel Hapgood, *The Karamazoff Brothers*. Hapgood attached that around 1905 to her English synopsis of a play based upon the novel.[10] It is possible that Garnett, in England, never knew of this little work of Hapgood; for I find no evidence that it circulated outside of New York, or that it is preserved except in the New York Public Library. But Hapgood's *Survey of Russian Literature*[11] also referred to *The Karamâzoff Brothers*, and was widely read. Still "The Brothers Karamazof" is found as early as 1886, in the introduction to Dostoyevsky's lesser novel *Injury and Insult*, translated by Frederick Whishaw (London: Vizetelly; p. x); similarly *The Brothers Karamazov* in Dmitri Merejkowski's *Tolstoi as Man and Artist*,[12] in A. Brückner's *Literary History of Russia* translated from German by H. Havelock,[13] and in Maurice Baring's *Landmarks in Russian Literature*.[14]

So Garnett did not originate the title; she merely adopted it, and it has been accepted by innumerable critics, whether they know the novel only through her or read it in the original. Her title became established, either because it connoted something richer in meaning than *The Karamazov Brothers* or merely because it was beyond reach of any correction, once published. After all, the title of a book -- like a person's name -- serves for a label; and therefore, as soon as we get used to it, as soon as we know the object it refers to, any incongruity ceases to matter.

Even the extreme incongruity of an untranslated title, such as *Les Misérables*, is quite tolerable. Of the several English translators who tried their hand on Hugo's novel after it came out in 1862, "the Maris Brothers," besides mentioning "the Landseer Brothers," an English pair.

9. First by E. Halpérine and Ch. Morice (Paris: E. Plon, Nourrit et Cie, 1888), later by J.-W. Bienstock and Charles Torquet (Paris: E. Fasquelle, 1906); *National Union Catalog* (above, note 4), 147, p. 429.
10. Ibid., 230, p. 339.
11. (New York: Chautauqua Press, 1902), pp. 218, 222, 225.
12. Translator anonymous (New York and London: G. P. Putnam, 1902), pp. 260, 274.
13. (London: T. Fisher Unwin, 1908), p. 409.
14. (London: Methuen, and New York: Macmillan, 1910), p. 240.

all kept the French title; and while two of them offered a parenthetical equivalent (The Wretched,[15] or The Outcasts[16]), neither one caught on. The most influential of the early versions were by Lascelles Wraxall and by Charles E. Wilbour, and were content with the poignant French words, which are indeed understandable enough to a monoglot English reader. He at least realizes that the words are French; so they suggest a particularly French sympathy for unhappiness.

Once Garnett's title *The Brothers Karamazov* was established in the English vocabulary, it became a model for translators of other books. So Maurice Samuel in 1936 took the Yiddish novel by I. J. Singer, די ברידער אשכנזי, and called it *The Brothers Ashkenazi*.[17] Both Singer and Samuel were obviously influenced by Dostoyevsky's masterpiece. I hesitate to say the same of *The Brothers Bellamy* by Humphrey Pakington (New York: W. W. Norton, 1953), since the story is so much lighter. However, *The Brothers Shubert* by Jerry Stagg (New York: Random House, 1968), a journalistic history of the theater magnates, is evidence that the syntagma became stylish.

A more extreme offshoot of *The Brothers Karamazov* is *The Family Moskat*, a novel by Singer's younger brother, Isaac Bashevis Singer. The Yiddish original, די פאמיליע מושקאט, was translated by A. H. Gross (New York: Knopf, 1950);[18] and the author, in a dedicatory preface, mentions *The Brothers Ashkenazi* by the elder Singer. The manifest sin against English idiom, which mandates *the X family*, is hardly conceivable without the background of novels entitled *The Brothers X* and that children's classic *The Swiss Family Robinson*, which is a maladroit condensation of the German title *Schweizerischer Robinson, oder der schiffbrüchige Schweizerprediger und seine Familie* by Johann David Wyss. An early translation was called *The Family Robinson Crusoe*.[19] Since the narrative carefully withholds their surname -- neither Robinson nor Crusoe -- it would not do to correct these titles to **The Robinson Family from Switzerland* or **The Robinson Crusoe Family*. A more meaningful rendering of *Schweizerischer Robinson* would have been 'A Swiss Robinson Crusoe'; but it is too late for that.

(b) *An Enemy of the People*

The history of another English title, *An Enemy of the People* for Ibsen's play *En Folkefiende*, is similar to *The Brothers Karamazov* in that at first it was called something more idiomatic, *An Enemy of Society*, in the version of 1888 by Eleanor Marx-Aveling. That is an

15. The anonymous A. F. (Richmond: West & Johnston, 1863-4).
16. Henry L[lewellyn] Williams (New York: Hurst [n.d.]).
17. *A Catalog of Books Represented by Library of Congress Printed Cards*, 137, p. 535; *Library of Congress and National Union Catalog: Author Lists, 1942-1962*, 125, p. 169.
18. Hebrew Union College -- Jewish Institute of Religion, *Dictionary Catalog of the Klau Library, Cincinnati*, 23 (Boston: G. K. Hall, 1964), p. 692. מושקאט would normally be transliterated *Mushkat*. Is *Moskat* due to mere sloppiness?
19. British Museum (above, note 4), 261, pp. 950-951.

exact rendering, not of the Dano-Norwegian title but of an important expression in the second act, "en samfundets fiende."[20] The hero's brother, Peter Stockmann, the mayor, infers that of him: "The man who makes such offensive insinuations against his own native place must be an enemy of society."[21] But at the climax, in the fourth act, when Thomas Stockmann tells the citizens of the town what he thinks of them, he evokes the outcry "en folkefiende," repeated over and over. Marx-Aveling translated that outcry as literally as she dared, 'an enemy of the people'; for *folk* in Norwegian = *people* and *fiende* = *enemy*, but we happen not to compound these two in the Norwegian fashion.

An apology accompanies the title in that first English edition (p. 199): "For the title of this play -- *En Folkefiende*, literally 'a folk-enemy,' or 'an enemy of the people,' -- no exact idiomatic equivalent can be found in English. 'An Enemy of Society' has seemed the most satisfactory rendering available." It appears that the phrase *a public enemy* was somehow missing from the active vocabulary not only of Marx-Aveling but also of the editor, Havelock Ellis. "En folkefiende" in that great scene failed to ring a bell. It is indeed a dramatic irony that Thomas Stockmann incurs such an insult for being really a public benefactor, telling the indispensable truth at whatever cost. William Archer's translation in 1890 followed Marx-Aveling identically in regard to "an enemy of society" and "an enemy of the people," and likewise made the former the title. But Archer's later editions come with the title *An Enemy of the People*, eliminating the inconsistency that had been introduced for the sake of smoother English.

None of the subsequent translators until Peter Watts in 1964 hit upon the right phrase, *A Public Enemy*.[22] Yet the Oxford English Dictionary quotes an example of it from 1845, in Ranke's *History of the Reformation in Germany*, translated by Sarah Austin (London: Longmans, 1845; II, 215), where it represents "ein öffentlicher Feind" in the German original. The idea of a public enemy, a citizen who turns against the whole community, came from ancient Rome; and in translations of the Latin classics the phrase was commonly used for *publicus hostis* or *publicus inimicus* (e.g., Livy 6.19.2; Augustine, *De ciuitate Dei* 2.20). Naturally it also appeared in English writings on

20. *Samlede Verker*, ed. by Francis Bull et al., IX (Oslo: Gyldendal, 1932), 242, 243.
21. *The Pillars of Society, and Other Plays*, ed. by Havelock Ellis (London: Walter Scott, 1888), p. 242.
22. *Ghosts and Other Plays* (Baltimore: Penguin Books), p. 294: "The usual translation of the title *En Folkefiende* is Archer's *An Enemy of the People*. I have broken with this tradition out of regard for the small-part actors in Act Four. I simply cannot believe in an angry crowd shouting 'Enemy of the People, Enemy of the People!' and even the 'capable actors' whom Ibsen demanded for these walk-on parts would have difficulty in making the phrase sound convincing. 'Public Enemy' comes more easily off the tongue." Bravo, but he still talks as if Archer's title, although unnatural, were somehow truer to Ibsen's meaning.

Roman history;[23] and no doubt it spread quite a bit beyond that field. But it could not have been widely familiar in the time that so many translators of Ibsen failed to think of it -- inhibited, to be sure, by the structural difference between the Norwegian compound noun and the English adjective + noun. Nowadays we would all know better, since the Chicago Crime Commission and then the F.B.I. in the 1930's dramatically applied the term *public enemy* to gangsters.[24]

Although an excellent knowledge of English is frequent among the Scandinavians now, Norwegian-English or Danish-English dictionaries in Ibsen's time left much to be desired.[25] They would not have helped his translators with the meaning of *folkefiende*. I suspect that the listings since then, both in monoglot and in bilingual dictionaries, include this transparent compound only because of the fame of Ibsen's play; for several of them refer to him. The bilingual ones kept defining *folkefiende* as 'enemy of the people,' evidently acquiescing in the customary English title, until Einar Haugen's *Norwegian English Dictionary* in 1965[26] gave the correct gloss 'public enemy.' But he added a capitalized citation: "En f- An Enemy of the People (Ibsen)."[27] It remains for the future to show whether Archer's title for it will continue to prevail or will give way to Watts' improvement.

(c) *The Deputy* and *The Marriage of Figaro*

Somewhat as Marx-Aveling, Ellis, and Archer did not quite understand *En Folkefiende* when they failed to equate it with 'a public enemy,' so when Rolf Hochhuth's play *Der Stellvertreter* (Hamburg: Rowohlt, 1963) was turned into English by Richard and Clara Winston (New York: Grove Press, 1964), they did not hit upon the right equivalent. *The Deputy*, to be sure, is not in itself unidiomatic, and in a different context it would do very well for the German word. But this drama is about the Pope as "Stellvertreter Christi"; and that, in English, is *vicar of Christ* or *Christ's vicar*, whenever the theme has been talked about before Hochhuth. The Winstons may never have known, or not remembered, the old literature of controversy between Catholics and Protestants over the Papacy. Or possibly they felt that *The Vicar*, standing alone as the title, would suggest a village parson of the Church of England, whereas *The Deputy* is more opaque

23. E.g., Conyers Middleton, *The Life of Marcus Tullius Cicero*, I (Boston: Wells and Lilly, 1818; first publ. in 1741), 355.
24. Mitford M. Mathews (ed.), *A Dictionary of Americanisms, on Historical Principles*, II (University of Chicago Press, 1951), 1324: "Frank J. Loesch . . . who added 'public enemy' to the nation's vernacular."
25. That province of lexicography was no longer in its infancy. Christian Fredrik Bay's *Compleat Vocabulary of English and Danish* had been published as early as 1784. Robert L. Collison, *Dictionaries of Foreign Languages* (New York: Hafner, 1955), p. 82; *National Union Catalog* (above, note 4), 40, p. 434.
26. Oslo: Universitetforlaget, and Madison: University of Wisconsin Press.
27. Cf. his translation (with A. E. Santaniello) of Halvdan Koht, *Life of Ibsen* (New York: Benjamin Blom, 1971), p. 338.

and unlikely to be visualized either as a member of the French parliament or as a sheriff's deputy in a "Western."

Anyhow they use *vicar* rather than *deputy* only once, and then for the German *Vikar* in a sentence of the epilogue where it is explicitly a synonym for *Vertreter*. Hochhuth quotes from a sarcastic comment of Mussolini, not in the original Italian but in German: "Gottes Vikar, das ist der irdische Vertreter des Herrn des Universums." The Winstons handle it accurately enough: "God's vicar is the deputy of the Lord of the universe"; but they show no awareness anywhere in the play that *vicar*, from the Latin *uicārius*, is normally the English term in an ecclesiastical sense for what the Germans, by their norm, have translated 'Stellvertreter.'

In English, because the language has been so receptive to foreign words, it is especially easy at times to transcribe instead of really translating. Beaumarchais's sensational comedy, *La folle journée, ou le mariage de Figaro*, was promptly translated by Thomas Holcroft as *The Follies of a Day, or The Marriage of Figaro*.[28] It is about Figaro's wedding day, and that would be a much apter title. **Figaro's Marriage* could be justified, since part of the action hinges on whether Figaro will get married, and to whom. Since the play is no longer popular, the conventional English title *The Marriage of Figaro* is now applied most often to Mozart's opera, in preference to its Italian name *Le nozze di Figaro*.[29] The librettist Lorenzo da Ponte, for lack of an Italian cognate to *mariage*, could not handle it in Holcroft's manner; instead he had to think what it means in the whole context of the drama, and he came up with *Le nozze* 'The Wedding' or 'The Wedding Day' rather than *Il matrimonio* 'The Marriage.'

In the long run English has been enormously enriched, and not gravely confused, by that very habit of the polyglot upper class to take over the foreign word and copy the foreign syntax, instead of troubling to find a vernacular equivalent. The many superfluous borrowings enjoyed, from the first, their special associations and nuances. Each one has gradually settled down so that it does not cover quite the same semantic field as it did in French or Latin or whatever, nor is it simply interchangeable with any native English expression.

(d) The Song of Songs

One title, *The Song of Songs*, no longer has an unidiomatic sound. It was a literal translation of the Latin *Canticum Canticorum*, and

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28. *National Union Catalog* (above, note 4), 41, pp. 562-563.

29. When operas are sung in the language of the original score, for the most part they will naturally be known by their untranslated title. There are exceptions for various causes: *A Masked Ball* is far less cumbersome to speakers of English than *Un ballo in maschera; Madame Butterfly* (pronounced as though it were *Madam)* was an English short story by John Luther Long about a Japanese girl and her American husband, and a successful English play by David Belasco, before Puccini took his opera *Madama Butterfly* from it, and for us the combination of the Italian *Madama* and the English *Butterfly* is too incongruous.

that in turn of the Hebrew שִׁיר הַשִּׁירִים.[30] The literalness, however, was no more than usual in Bible versions until the present century; the translators felt bound by the received wording of the sacred text, much more than by the idiom of their own language.

Hebrew has no direct counterpart to the superlative of the Indo-European languages and Arabic; {šíʸr haš·iʸríʸm} may serve for nearly the same meaning as *the greatest* or *the finest of all songs*. The logic of the Hebrew idiom is easy to see in מֶלֶךְ מְלָכִים {mélək mlokíʸm} 'king of kings' (Ezek. 26:7), referring to Nebuchadnezzar, whose empire embraced many kingdoms, and in the opposite עֶבֶד עֲבָדִים {əébəd əăbodíʸm} 'slave of slaves' (Gen. 9:25), the curse upon Canaan to be the lowliest of underlings. But Hebrew usage is not limited to such cases; the notion of 'the ultimate in song, the song *par excellence*' is expressed the same way. An alternative rendering of {šíʸr haš·iʸríʸm} might be 'The Anthology' -- i.e., the song compiled from pre-existing songs; that too would be, in its way, by selection, the finest of all songs.

But readers of the Bible in translation have not understood *The Song of Songs* to mean 'The Anthology.' Instead it has, along with *vanity of vanities* and *holy of holies*, engendered a syntagma in the languages of Christendom. *Wonder of wonders*, or *miracle of miracles*, is not in the Bible, but it has a Biblical ring. *Horror of horrors*, *night of nights*, *pearl of pearls* connote something awesome. So what was originally alien to English has become established, and within a limited range productive.

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30. Because Latin, the intermediary language, had no article, we are spared the embarrassment of too literal a rendering of the Hebrew: *'Song of the Songs.' English idiom differs greatly from Hebrew in regard to the article.

ANGLO-FRENCH MISLEADING TRANSFER ITEMS:

A SEMANTIC ANALYSIS

Jean-Luc Garneau

Lake Forest College

In his book The Origins and Development of the English Language, Thomas Pyles reminds us that "no loan-words unquestionably of French origin occur in English earlier than 1066". And E. H. Sturtevant in his Linguistic Change says that "for centuries after the Norman Conquest vast numbers of French words were naturalized".

It was not the first time in history that one witnessed such a socio-linguistic upset due to conquest. Under the Romans the language of the Gauls had also undergone such an overwhelming influence. But the reason we stress this socio-linguistic event is that these loan-words, after having been naturalized, have evolved independently of the mother language, have flown with their own wings and have acquired meanings which very often make them difficult to identify, at least sememically, as being the same lexemes with the same etymology.[1]

Because of the great number of such pairs between the members of which a semantic breach has arisen, an important problem now exists. This problem has been may a time approached by different traditional grammarians without their trying to identify the types of semantic divergence at work between these Franco-English pairs of words. These words which were historically bonafide cognates have become, as we analyze them in synchrony, Misleading Transfer Items[2] causing much upheaval for the learner of either French or English.

It is often said that once the existence of a problem is recognized, it is as good as solved. I am afraid that such does not seem to be the case in the situation above and despite the numerous and courageous battles fought against those enemies traditionally called "faux-amis" by a great number of teachers, translators and linguists, the problem is far from having been solved. Students are still being misled by this stumbling block par excellence.

Scarcely an academic term goes by without at least one young lady telling me of her weekend romantique, with the Romantic movement in literature being far from her thoughts, but being the only thought conjured up in the

mind of an unsuspecting Frenchman unfamiliar with English. Students speak of a journée of 100 miles and of having to travailler for 2 days to arrive at their destination; they mean, of course, a 'journey' and 'to travel' respectively.[3]

The problem is, of course, that etymologically linked couplets have developed divergent meanings, while retaining similar orthographics, and is complicated by the fact that there is no obvious rule of thumb to distinguish them from Reliable Transfer Items. The confusion crosses all cultural facets, all levels of language and in-depth study of French and English does little to clarify the situation. This is not to infer that all is hopeless. Rather, I find that there are indeed some clearly definable categories of semantic change which these cognates have undergone.

1.0 Concrete Abstract Progression

One overwhelmingly obvious trend in language evolution is the move from the concrete to the abstract called Kronasser's Law.[4] An application of this to a synchronic analysis of Misleading Transfer Items leads to the realization that often one member of a couplet as compared to the other member stands in one of two positions. The first of these is that one member be applicable to the concrete only, while its partner member applies only to the abstract, e.g. tutor/tuteur, the former being indicative of the caring for the mental well-being of an individual and the latter for the physical well-being; adjectives affluent/affluent, the first meaning 'rich, opulent,copious, flowing freely' and applying only as an abstract, while the second refers only to the concrete 'free flowing of water'.

A second kind of Concrete Abstract Progression is typified by one member of the couplet being applicable to only the abstract or to only the concrete, but not both, e.g. plateau/plateau, the former being used to describe either a geological table-like formation or a leveling-off, a relatively quiescent state, whereas the latter conveys only the geological meaning; profound/profond, the first referring to abstract depth, as of ideas, only, and the latter having that same use, but also the concrete sense as of deep water.

2.0 Coexistence

Michel Bréal reminds us that both old and new meanings of words coexist for at least a limited time before, if ever, the old drops out of use.[5] Such is the case with MTI control/contrôler. Fr. contrôler < L contra 'against' + rotulus 'wheel' and signifies 'to verify'; Eng. control has the same origin and means 'to regulate'. But the English has added a more abstract element, 'to exercise authority

over', which the French understand by maîtriser, circonscrire, and to which contrôler does not apply. Expanding this theory to synchronic application widens the range of MTIs that it characterizes and brings it to ressemble semantic widening and narrowing, interference and extension.

2.1 Extension

Vinay and Darbelnet (Stylistique Comparée, 1964) rename the phenomena of semantic widening and semantic narrowing and call them 'extension', qualified as greater or lesser, and apply it to bilingual study, French and English in particular. While they do not by any means restrict extension to etymologically linked couplets, and indeed apply it to any dictionary entries which translate each other, and sometimes only marginally at that, it is coincidental that the example they use is an MTI couplet: Eng. clerk, they state, has greater extension than Fr. clerc, because clerk has two essential meanings, i.e. 'a white collar worker' and 'a salesman', of which Fr. clerc is the translation of only one, 'a white collar worker', and furthermore 'a white collar worker in government offices' only. Their distinction between greater and lesser extension is one of convenience of speech: clerk has greater extension than clerc, and clerc has less extension than clerk, depending on the point of view desired.

Vinay and Darbelnet credit DeSaussure with having already mentioned this semantic notion of extension under the title of valeur.[6]

Interference describes such cases of narrowing as those wherein a couplet member carries narrower meaning than does its partner because a synonymous term or terms has helped carry the load of its partner's larger meaning. An example is MTI country/contrée < L contrata. Eng. country signifies: 1) 'a tract of land'; 2) 'a nation'; 3) 'places outside of the city'. Fr. contrée corresponds only to the first, pays is used for the second, and campagne for the third. The appearance and popularity of pays and campagne have obviously contributed to the narrowed meaning of contrée. In MTI plume/plume L plume 'feather', the very presence of the words pen, writer and feather interferred with the use of Eng. plume, whereas Fr. plume has retained or developped them as part of its meaning.

Semantic widening and narrowing are not easily stretched to describe a bilingual study and extension and interference are not limited to etymologically linked couplets. Bréal's heading of coexistence is most easily adaptable[7] and will designate in this study those couplets whose members

can be characterized as above under any of the particular descriptions.

The relationship between the member of a couplet characterized as affected by coexistence and the other so-called coexisting lexemes of the same language is not necessarily causal, i.e. the popularity of pays and campagne may or may not be the cause of the limited use of contrée: this would be best handled in a diachronic way. For my stated purposes, it will be most useful to simply arouse the awareness of the fact of coexistence.

2.2 Divergent Coexistence

Some MTI couplets are so steeped in the confusion of coexisting lexemes that the resulting analysis appears to be a labyrinth. MTI piece/pièce is one such example: Eng. piece is translated in French by morceau when it means part of a substance that can be cut or broken into parts of divergent size and shape, such as wood, cardboard, cotton, apple, metal or plastic; it is translated tranche when the shape is slice-like in shape, as with bread or cake; barre when the shape is square, as with soap or chocolate; the French say façon for 'piece of one's mind' and fusil for military purposes; there is also a sexual connotation conveyed at times with Eng. piece which requires a grouping of words as translation in French. Pièce, on the other hand, is best translated patch, room or theater play and rarely, if ever, carries the same meaning as Eng. piece. Because the coexistence of lexemes comes into play on both sides of the couplet, I call this divergent coexistence and find it to be one of the more difficult distinctions of vocabulary for the learner to grasp and retain.

MTI part/part, while not a case of divergent coexistence, does fully qualify as an item under the heading of single coexistence as the following explanation will show: Eng. part can be translated into one of at least 6 French lexemes which are: partie as when describing the parts of the body; portion when the parts of the whole to be divided can be measured with much the same distinction as between the Eng. part and portion; morceau, for when Eng. part is synonymous with Eng. piece; région for the geographical application of Eng. part which is not easily identified in terms of boundaries; pays when that geographical application does have clear boundaries; rôle for 'a part in a play'. Curiously enough, Fr. part means only a 'share of stock', with the exception of 'la part du lion', 'the lion's share'.

Some simpler examples of coexistence are: MTI candle/chandelle, the latter indicating only those candles made of bee's wax, and bougie being the term for

all synthetic candles; MTI money/monnaie, the English member referring to currency in general, while the French is limited to coin or small change only; MTI pest/peste, of which the French could be said to have greater extension since English must use the more precise plague to designate the rampant disease which the French covers in one stroke; MTI parents/parents, in English referring to mother and father only, but in French signifying either the same or more generally all family relations, i.e. relatives, the distinction to be made from the context in which the word is used; MTI property/propriété, the two being identical except that propriété also means exactly what it appears to: 'propriety'; MTI prize/prix, in which the French has greater extension due to the fact that it carries the meaning of 'price' for which, obviously, the English has a separate lexeme.

3.0 Borrowing for Specific Meaning

3.1 Borrowing

One primary means of expanding the vocabulary of a language is by borrowing the lexemes of another language[8]. Between French and English the most frequent borrower, at least up to the two World Wars, has been English.

3.2 Cultural Loans

The English vocabulary of some cultural subfamilies, such as culinary, couture and theater, is rampant with instances of French lexemes borrowed for single specific meanings; most borrowing for specific meaning has in effect gone from French to English, but in the past 50 years, since the facts of American life and civilization have become so preiminently present in France, several cases can be found in which the specialized term has been borrowed from English.

Obviously all cases of borrowing are not cases of Misleading Transfer Items; furthermore, those cases which are indeed MTIs are a particular type of extension, i.e. that the member of the couplet which has been borrowed has less extension than the other member. This could be classified under Coexistence with the subheading of 'Specialization', but due to the large volume of appropriate MTIs, a separate heading is called for.

One case of borrowing from English for a specialized meaning in French is starter, an automotive term meaning 'choke of a car' in France, and resulting in the MTI starter/starter. The automotive sense of Eng. starter, besides being different in meaning, is not the

only use of the word: the others are 1) 'one that starts', 2) 'a worker who supervises departures of public transportation vehicles', 3) 'self-starter' (automotive), 4) 'one who signals the start of a race', and 5) 'a person or animal that starts in a race'. MTI ticket/ticket is an example in which the borrowed French meaning is identical with part of the meaning of the English lexeme, but not synonymous with the entire semantic span of the English lexeme. The specialized meaning of ticket (and the only one in French) is that of a piece of paper signifying permission to enter, such as a plane ticket, a theater ticket, and does not include 1) 'a traffic ticket', which is called contravention, 2) 'a captain's or pilot's license', 3) 'a label', 4) 'a political slate', or 5) the slang as in 'a new hat would be just the ticket for her', meaning 'the proper thing'.

But, to repeat, the great majority of these cases are found in borrowing from French to English.Thus, MTI veal/veau illustrates this type of borrowing. English adopted veal to designate the meat processed from calves, which is one of the meanings of veau, but does not use it to signify the calf itself, which is the primary meaning of veau. Analagous couplets are MTI mutton/mouton, the French being not only the meat, but the lamb itself; MTI beef/boeuf, the French being the term for both the bull and the beef.

The English farce is a literary genre, the same as in French, but there it connotes first and foremost a 'joke' (MTI farce/farce). A military sortie is a particularization of the French word for 'exit' to mean 'the flight of an airplane on combat mission'; the French would use additional determiners or depend upon the context to so particularize the meaning of sortie, which is essentially an unqualified 'going out'.

Couplets in which the common meaning of the members is reliable, but in which the French member has more ordinary, frequent, or simply additional uses follow. (I have indicated these latter in parentheses.) MTI framboise/framboise 'a French raspberry brandy' (in French simply 'raspberry'); MTI tableau/tableau 'a vivid or graphic description, in theatre a scene in which the actors on stage freeze in position' (a painting, a blackboard); MTI voile/voile 'a sheer fabric of cotton, rayon, silk or wool used for making curtains, dresses or the like' (masculine: 'veil'; feminine: 'sail'); MTI toile/toile 'a sheer linen fabric' (theatre: 'curtain or backdrop'; nautical: 'sail'; art: 'canvas'; 'spider's web'); MTI franchise/franchise 'privilege granted a person or group by a government, state or sovereign' ('frankness').

4.0 Bifurcation of the Original Meaning

Without going into a diachronic analysis of the evolution of bonafide cognates into Misleading Transfer Items in French and English, it can be pinpointed, as a mnemonic device to the student, that the best description one could give of the semantic difference existing in synchrony between partners of some MTI couplets is to say that they have undergone a bifurcation in their original meaning.

MTIs exemplifying this semantic transfer type are: 1) journey/journée, in which case the English means 'a trip' while the French means '24 hours'. The etymology of the MTI couplet -- VL diurnāta < L diurnum 'daily portion' which came to mean in ME journey 'a day's travelling' -- gives us the explanation for the synchronic semantic breach existing between journey 'a trip' and journée '24 hours'.

2) In the case of MTI travel/travailler < ME travailer 'to toil, to make a (toilsome) journey' < OF travailler 'to travail'. Travel, like journey, is synonymous with a 'trip' whereas travailler literally means 'to work'. From the ME travailer, English carries the sense of 'journey' and French retains the 'toilsome' aspect, i.e. 'work'.

3) MTI sophisticated/sophistiqué < Med L sophisticāre < sophisticus 'captious; devious, skillful deception' < Gr. sophistikas < sophistēs 'sophist': sophisticated carries the meaning 'having acquired worldly knowledge or refinement; complex or complicated; refined' and can be said to have retained the Latin sense of 'skillful' while the French sophistiqué has gone the way of 'deception' and means 'that which has been falsified' in the same way as English sophistic.

4) MTI parson/personne, the former indicating a 'clergyman' and the latter a 'person', taking source in Latin persōna (ecclēsiae) 'person (of the church)'; the English obviously retains the ecclesiastic character of its source, while the French prefers secularization and generalization.

5) MTI concourse 'a great crowd, a flowing together, a large open space for the gathering or passage of crowds' / concours 'competitive examination, competition, rally, show' < L concursus past participle of concurrere 'to run together'; the English carries the meaning of 'together' applying it still further to the place of togetherness, i.e. 'large open space for gathering of crowds', while the French emphasizes the 'run' with 'competition, rally'.

6) MTI traduce 'to slander, defame'/traduire 'to translate' < L trādūcere 'to lead across, make public, expose to ridicule'; the Latin signification of 'to lead across' aptly describes the act of translation, i.e. the French couplet member, whereas the English is a further malicious extension of 'to expose to ridicule'.

7) MTI vent 'a means of escaping' /vent 'wind, air' < VL exventāre < CL ex 'out' + ventus 'wind'; the split is obvious -- Eng. vent carries the meaning of CL ex 'out' and Fr. vent is most literally 'wind'.

5.0 Connotation

There is a large number of Misleading Transfer Items couplets whose problematic difference in meaning cannot be decisively pinpointed, largely because they are used, for the most part, in apparently identical syntactical environments, as well as related if not identical cultural environments. The question here is of a type of extension in the sense that one member of the couplet carries additional meaning in the form of connotation which the other member lacks. However, the connotational aspect of the member concerned is not easily discernible in the context in which it is used. In some cases, such as with sexual connotation, the additional meaning is often reinforced with intonation or gesture and in other cases the connotational meaning is instinctively present in the mind of the native speaker and native listener. Because the presence of a connotational meaning borders on idiomaticity, i.e. the whole equals more than the sum total of all of its parts, it merits special consideration apart from any of the aforementioned categories of semantic transfer types. Dictionaries, even when used comparatively to seek out divergent meanings of etymologically linked couplets, provide virtually no clues to the presence of connotational differences in meaning, something which cannot be said of idioms.[9]

5.1 Derogatory Connotation

One of the members of the following MTI couplets carries a derogatory connotation. In the case of MTI base/bas, the derogatory connotation is attached to base 'of low birth, servile' while bas means 'low' the contrary to 'high'. If we consider the MTI beak 'bill of a bird', 'person's nose' /bec 'bill of a bird', we find that here again it is the English member of the couplet which connotes the derogatory effect, that of being used to designate a person's nose. The MTI tripe/tripe presents us with another pair of Anglo-French lexical items for which the difference in meaning shows up again in that one member, the English once more, is marked with the feature of conveyance of derogatory meaning. Thus Eng. tripe means 'lining of the stomach of cattle used as food', a meaning which is also

conveyed by Fr. tripe, but the Eng. tripe also means 'anything of no value, rubbish' and this is what gives Eng. tripe its derogatory connotation. Finally, in the case of petty 'spiteful man' / petit 'little, small', the lexeme petty connotes derogatory effect. One can say that this derogatory connotation, in the cases studied above, is inherent in the words concerned and that even when used innocently by the untutored, they convey the derogatory effect.

5.2 Sexual Connotation

The same statement cannot be made about sexual connotation as observed in the following couplets: MTI intrigue 'clandestine love affair' / intrigue 'plot', (E); MTI ball 'spherical object, testicle' / balle 'spherical object used in games' (E); MTI facile 'easy, fluent, superficial' / facile 'easy, of easy virtue, ready' (Fr.). The fact that some of these entries carry the sexual connotation only in certain cases makes them valuable for idiolectal use by the forgers of double-entendre jokes. And when misused by the uninitiated, these complex items are therefore less of an affront to the native listener.

In MTI massage 'the rubbing of parts of the body to aid circulation and relax the muscles' / massage with the same meaning, the English word conveys a sexual connotation when it is juxtaposed to parlor.

5.3 Euphemism

A euphemistic connotation[10] is present in the MTIs chanteuse 'nightclub singer' / chanteuse 'woman singer, vocalist' (E); MTI derriere 'buttocks' / derrière 'backside, behind' (E) [11]; MTI epaulette 'shoulder ornament, especially two fringed straps on certain uniforms' is a glorified use of the Fr. épaulette 'shoulder pad'; also MTI chiffon 'diaphanous silky muslin' whose French counterpart chiffon signifies 'rag'.

5.4 Connotation of Purposefulness

A connotation of purposefulness identifies the difference in meaning between members of MTIs deceive 'to delude, mislead' / décevoir 'to disappoint' (E) and ignore 'to refuse to pay attention to' / ignorer 'to be ignorant, unaware of' (E). Fr. décevoir is not of necessity purposeful, whereas Eng. deceive connotes the pre-conceived intention to trick someone.

5.5 Connotation of Spacial Differences

The trouble-causing element in MTI large 'of considerable size, important' / large 'wide' stems

from the fact that both members express different dimensional connotations. In English, large connotes overall dimensions and in French the dimension emphasized is width. Similar cases are: 1) MTI abyss 'bottomless pit' / abysse 'great underwater depths', the English referring to an immeasurable depth, while the French insists only on the greatness of the depth; 2) MTI pilule, 'pill' / pilule 'pill', the English lexeme including only the pills of small sizes, while the French word covers pills of all sizes.

5.6 Connotation of Religion

There is a certain number of couplets one member of which carries a religious connotation which is not present in its partner member: MTI beneficial 'advantageous' / bénéficial 'ecclesiastical profit' (Fr.); MTI piscina 'sacrarium' / piscine 'swimming pool' (E); lavabo 'ceremonial and religious washing of hands' / lavabo 'sink,lavatory, washroom and also religious washing of the hands', which is the only meaning conveyed by the English lexeme; redemption 'repurchase, rescue' / rédemption 'redeeming' (Fr.).

5.7 Contrasts in Speech Direction

To define the sememic difference between the two members of an MTI couplet as a contrast in speech direction is to say, laconically, that those two members move along the same road in opposite directions. The trajectory is the same for both members, but is directionally different. MTI sympathetic/sympathique is a prime example. He is sympathetic marks the movement of sympathy from the subject he to an animate or an inanimate. Il est sympathique marks a movement of sympathy from the speaker to il, an animate only, and the sentence means 'he is nice'.

In like manner, MTI lecture/lecture is a case of contrast in speech direction. The Eng. lecture means 'the reading of or presenting of a paper to a group of listeners', and its French equivalent is conférence. In French, lecture is 'reading to oneself', the English translation being reading. The same actions are involved; the direction in one case from the reader to the listeners, and in the other from the reader to himself, is different. Other examples of contrast in speech direction are: MTI agreeable 'ready to consent' / agréable 'pleasing, acceptable'; complaint 'expression of dissatisfaction, grief' / complainte 'lament'; MTI relax 'to rest' / relaxer 'to release, to discharge'.

5.8 Connotation of Degree

The connotation of degree, which can sometimes be more precisely called connotation of excess or intensity,

is the semantic transfer type which best describes the difference in meaning between such couplets as MTI candid 'frank, free from bias' / candide 'gullible, credulous' (Fr.); MTI decry 'to belittle, to depreciate' / décrier 'to malign, to slander (Fr.); MTI infant 'a baby' / enfant 'child' (E) MTI retarded 'relatively slow in mental or emotional development' / retardé 'delayed, postponed' (E).

In most of these cases, both members seem to have the same meaning, except for a connotation of degree or excess which one member picks up and carries above and beyond that of its partner member. The common semantic feature is exhibited to a fault by the trouble-causing couplet member.

6.0 Social Register of Speech

One finesse in the usage of a language is knowing exactly when to use one word in the place of another when both carry identical meanings. The question is one of propriety, of using the common word for common occasions and the 'fancier' word for more ceremonious events. An example within English alone would be the choice of gravy instead of sauce: both are, ingredient-wise and in terms of preparation, one and the same fact of cooking; the choice depends on the occasion, such as gravy for the folks, but sauce for the boss.

The same choice is often to be made between two languages. A Frenchman must learn exactly when it is that his sauce is translated sauce and when it is not. This brings us to the semantic transfer type I call Social Register of Speech. It is at the heart of many an error in the speaking of a second language, as the following cases will exemplify. Of course, the effect of such misuse is not one which renders speech incomprehensible, but it is one of the fine points by which we distinguish a fluent speaker from one who 'manages to get by' in another language.

The essential difference between members of the following couplets is that the one I will indicate could be said to pertain to usage at a higher level.

MTI voyage/voyage (E), the former being of greater stature than a mere 'trip' which is the prime meaning of the French; MTI tempest/tempête (E) in English a 'violent windstorm, normally at sea' which is much more than a 'storm' as in French. No mother ever said to her child: 'You can't play outside today, Johnny, there's going to be a tempest'. The word carries simply too high a level of meaning to be of everyday use. MTI rendez-vous/rendez-vous (E), in French a simple 'appointment', something one has with a dentist, a hairdresser, a lawyer, with nothing romantic in mind; in English, rendez-vous signifies immediately a setting quite different from an office, rather a small

café, a corner in the park, at which something out of the ordinary will occur -- the encounter of two spies, the meeting of lovers, the site of a great battle. The ordinary dictionary is unprepared to indicate the presence of a different level of language for the untutored; the student of language has no place to turn to such information.

7.0 Figures of Speech

In a manner parallel to the use of figures of speech within a single language, metonymy, synecdoche and other figures of speech can be distinguished between the couplet members studied here in English and French. The distinction must be made, however, between figures of speech in bilingualism and in single language contexts, inasmuch as while they are useful as poetic elements with one language, they are a source of troublesome differentiation of meaning in bilingualism. Sturtevant in his Linguistic Change (1965) devotes a good deal of effort to showing how change is brought about through the use of 'worn-out figures of speech', in a single language situation. I will use them here to illustrate once again the type of semantic difference between members of MTI couplets.

Metonymy, the use of the name of a thing for that of another associated with or suggested by it, i.e. 'she sets a good table' for 'she is a good cook', can be seen as the difference between the members of MTI layette/layette. In French, there is the meaning of a 'very light-weight wooden box' in addition to the meaning which it has in common with English, i.e. 'clothing and equipment for a newborn'. It can be argued, therefore, that awareness of the presence of the 'box', once used to contain the baby clothes in French, the fact of metonymy between one meaning in French and the sole meaning in English, will help clarify the problem of difference in meaning here for the student of language.

Another case of metonymy is found in MTI mail/malle the latter being the container in which mail is carried and the former being the mail itself.

Synecdoche is the naming of a thing for one of its parts or qualities. This can help explain the particular type of limiting of meaning present in the following: MTI grape/grappe the former, besides its specified botanical signification, indicates the fruit of a vine, while the latter signifies the cluster of such fruit; MTI figure/figure, in French referring to the face and in English concentrating mainly on the lines of the body; MTI paper/papier, the latter referring to the substance made from wood-pulp used chiefly for writing and wrapping, and the English covering also the content of what is written on that substance, paper being synonymous with 'dissertation, thesis'.

Another figure of speech which helps clarify the semantic differences between these couplet members is the use of the animate for the inanimate and vice-versa. For example, in MTI massacre/massacre, the French member can be used, besides the common use of'slaughter', to indicate a person who is an 'inefficient worker'; in MTI puppy/poupée, the French refers to the inanimate only, a 'doll', and the English to the animate only, i.e. 'a small dog'; MTI establishment/établissement differ in that the English has recently taken on a reference to the people who make up the group running the 'establishment of life'. This meaning is slowly making its way into the French language.

8.0 Conclusion

As this study has shown, the complex semantic transfer types found within one language studied as a mono-system can also be discovered between two language systems borrowing from and influencing one another.

As pinpointed by Professor Halliday in the opening sentences of the paper he read at the First Lacus Forum, 1974, modern linguists have forgotten that "Man doesn't talk, men talk. People talk to each other". And we can add that people talk in many different languages.

It becomes obvious that a better understanding of human language could be reached if linguists studied more closely the intersystematic rapports which exist between different languages. In this matter, we have much to learn from the translators.

Footnotes

1. For the morpheme-lexeme-sememe distinction and definition of these terms, see Lamb (1966), Lockwood (1972), Makkai (1972) and Makkai and Lockwood (1973).

2. Misleading Transfer Item will be used to connote pairs of cognates which are not at all identical in meaning, or which are only partially identical in meaning. The abreviation MTI will precede each couplet mentioned. The order of languages within an MTI couplet will be English, then French, separated by a virgule (/).

3. For more examples of this type, see Embarrassing Moments in French and How to Avoid Them, a Practical Handbook for Students and Tourists by Seymour Resnick and Doriane Kurz, (1953).

4. "Es lassen sich nur sehr wenige Wörter aufzeigen, deren Bedeutung bei genauerer Untersuchung wirklich nur Beziehungen zun Seelischen hat. Es sind so wenig, dass der Schluss unbedingt sehr nahe liegt, dass zunächst nur die sinnlich wahrnembaren Gegenstände bezeichnet wurden." Kronasser H. (1952) Handbuch der Semasiologie. Kurze Einführung in die Geschichte, Problematik und Terminologie der Bedeutungslehre Heidelberg.

5. "It would not ... be accurate to treat words as signs which dissappear at one stroke ... The new meaning of a word, whatever it may be, does not make an end to the old. They exist alongside of one another." Michel Bréal, Semantics. 1954.

6. DeSaussure, Cours, 1966: Français mouton et anglais sheep ont la même signification "mais non la même valeur et cela pour plusieurs raisons, en particulier parce qu'en parlant d'une pièce de viande apprêtée et servie sur la table, l'anglais dit mutton et non sheep."

7. Bréal discusses the phenomenon which I named coexistence here under the heading of polysemia in Semantics p. 139 (1954).

8. "Every speech community learns from its neighbors." Bloomfield, Language, p. 445. "The simplest kind of influence that one language may exert on another is the borrowing of words." Sapir, Language, p. 193.

9. On idioms in general, cf. Makkai (1972).

10. "Un autre moyen d'éviter un mot choquant est offert par la traduction. Ce qu'on ne peut pas dire dans sa propre langue on le dit dans une langue étrangère." Nyrop, 1913, Grammaire Historique, p. 264. cf. also Stern,

1931, Meaning and Change of Meaning: "A foreign word is substituted, which, being less definite in meaning, is in the same degree less offensive."

11. Derrière is cited by Nyrop (1913), p. 295 as being an euphémisme de décence in French: "on dit ... en français 'les parties postérieures, le derrière, le fondement, pour éviter les mots plus crus."

12. Vinay-Darbelnet, p. 11 of Stylistique comparée, speak of niveau de langue for what is called here Social Register of Speech, and define it "caractère stylistique d'une langue d'après le degré de culture de ceux qui la parlent." For further discussion on this topic, see also M/A/K/ Halliday, First Lacus Forum, 1974.

13. Gustaf Stern, in Meaning and Change of Meaning (1931) defines figures of speech as "intentional transfers which involve emotional factors. They are intended to serve the expressive and purposive functions of speech better than the plain statement."

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THE ASSIMILATION OF ENGLISH LOANWORDS IN ITALO-CANADIAN

Gianrenzo P. Clivio
University of Toronto

This paper presents a few preliminary results of a field project on the Italian language in Canada which was begun in the Summer of 1974. While definitive results are not yet available, it is hoped that the issues raised may be of general interest. A pilot project was carried out in the city of Toronto where, on a total population of over two million, approximately one person out of five is of Italian descent. In the case of Toronto, Italian descent generally means Italian speaking because immigration from Italy to Canada is on the whole quite recent and, in any case, has taken place almost exclusively after World War II.

It is important to mention at this point that Italian immigrants in Toronto are almost all dialect speakers, many of whom had acquired only a limited command of standard Italian in elementary school while living in communities where a local dialect was practically the only normal medium of communication. In Toronto, the dialects are the home language of almost all Italian families, with the result that -- while parents can to a greater or lesser extent speak Italian as well -- their Canadian born children learn only the dialect of their ancestral village or town. Although partly inadequate to cope with the expressive demands made by an urban and technological culture radically different from the rural or semirural environment from which most immigrants came, the dialects continue to be quite vigorous in Toronto. This is due, at least in part, to the fact that very often a number of families (sometimes several hundreds!) have immigrated from the same locality. Even in the new surroundings, ties of kinship and friendship are normally retained. Thus, there continue to be many opportunities to speak one's native dialect even outside of the immediate family.

Italy's profound dialectal fragmentation is a well known fact; often, considerable linguistic distance exists even between adjacent localities and dialects of different regions are normally not mutually intelligible or only partly so.

The influence of English on Italian manifests itself, above all, at the lexical level. This is in perfect accord with the findings in nearly all language contact situations, which show that of all the major components of a language system the lexicon is the one which can most easily undergo modifications and changes. When lexical change occurs through borrowing, it often takes the form of a mere series of additions to the existing lexical inventory already available to speakers of the language. Contrary to the phonemic system, which is made up of a limited and well definable set of correlated units, the lexicon of a language consists of thousands of lexemes and is open-ended so that additions made to it do not ordinarily involve structural modifications (although on occasion certain semantic families may undergo significant restructuring). Lexical changes, however, may also take the form of substitutions whereby, due to the influence of a prestigious language, certain items of the dominated language come to be discarded from its lexicon and are replaced by loanwords. This process does not occur abruptly. For a period of time -- which cannot be precisely specified -- both the old and the new lexeme coexist

as synonyms, perhaps gradually acquiring extralinguistic connotations (e.g. one may be felt to be more elegant than the other). Eventually, the older word comes to be used less and less until it becomes completely obsolete and may not even be intelligible any longer. In other instances, semantic specialization may occur so that the native word and the borrowed term, which were initially synonyms, each acquire a separate, though generally related meaning.

The two main processes of lexical change which can be observed in language contact situations are thus: 1) addition, and 2) substitution. While the first may well be regarded as occurring in response to a need, i.e. the acquisition of a suitable term for a new concept, this is often not so, since addition may be only preliminary to eventual substitution of an existing lexeme. The distinction between the two processes is thus not a very sharp one.

Examples of lexical addition in Italo-Canadian are such items as /ad:óge/ 'hot dog' and most of the terminology relating to automobiles (/kár:o/ 'car', /ab:ím:i/ 'high beams', /snotáiri/ 'snow tires', etc.) which, though it obviously exists in standard Italian, was not known to Italian immigrants. Even the word for 'telephone' has been borrowed from English because it was an unfamiliar object (the Italo-Canadian term is /telefóno/ as opposed to standard Italian /teléfono/). Numerous other words designating local Canadian institutions have been borrowed and constitute lexical additions.

Instances of lexical substitution are also very numerous: examples are /búk:o/ 'book', /kék:a/ 'cake', /méč:i/ 'matches', /puš:áre/ '(to) push', etc., all of which replace preexisting Italian lexemes (libro, torta, fiammiferi, spingere). These, or their dialectal equivalents, are still available to most speakers as synonyms of the new terms, but tend to be less commonly used.

Lexical substitutions of this kind raise a number of interesting theoretical issues. It is easy to understand that immigrants may find themselves unable to talk about their new environment using only the resources of their native language, since many objects, institutions and customs are unfamiliar to them. As a result they borrow lexemes from the local language to fill the empty slots in their vocabulary. Such borrowings may well be regarded and designated as necessary loans and they are generally easily explained. As a rule, necessary loans constitute cases of lexical addition. Lexical substitutions, on the other hand, do not appear to occur because of a demonstrable deficiency in the lexicon of the recipient language and they may be termed luxury loans. Clearly, the motivation for the introduction and acceptance by speakers of luxury loans is primarily extralinguistic, in the sense that the surrounding reality could be equally well inventoried without them. It is, at least in part, the prestige of the dominant language which leads speakers of the dominated one to accept and adopt lexical additions. The problem which arises here, and which cannot at present be answered satisfactorily, is why certain items are borrowed and others are not. In Italo-Canadian, for example, the verb /puš:áre/ 'to push' has become firmly established at the expense of the native spingere, but its opposite, 'to pull', has not been borrowed and a predictable /pul:áre/ does not occur.

In Italo-Canadian, luxury loans appear to constitute the majority of borrowed lexemes. However, in classifying Italo-Canadian loanwords, one must constantly keep in mind the social and linguistic background of the immigrants and consider as terms of reference not only standard Italian but also the dialects and the differences which exist between these. Thus, the existence of a standard Italian equivalent for a given English lexeme borrowed by Italo-Canadian is not an automatic reason for regarding such a word as a luxury loan.

Another useful method of classifying lexical borrowing is based on the distinction between morphemic importation and morphemic substitution. Importation occurs when a foreign morpheme is accepted and more or less integrated into the system of the recipient language. The words /búk:o/ 'book' and /méč:i/ 'matches' are instances of borrowing in Italo-Canadian involving morphemic importation. In other instances, however, lexical interference manifests itself by means of morphemic substitution. For example, English 'downtown' has originated the form /bás:a čit:á/ in Italo-Canadian which has replaced the standard Italian expression centro della città. For loans such as this, Haugen has proposed the term loanshift, which applies also to cases in which a word changes or extends its semantic content or range due to the existence of a similar sounding term in another language. In the case of English-Italian contact, this process generally involves items which are historically cognate. Consider the following examples:

Italo-Canadian	Standard Italian
accidente 'accident'	incidente
appartamento 'apartment building'	palazzo, edificio
applicazione 'application'	domanda
compressa 'compressed air shovel'	martello pneumatico
correre 'to run in a political campaign'	presentarsi alle elezioni
fattoria 'factory'	fabbrica
forma 'form'	modulo
fornace 'furnace'	bruciatore
fornitura 'furniture'	mobilia
lavorare 'to work, function'	funzionare
passare 'to pass (an exam)'	superare
sciabola 'shovel'	pala, badile
segnare 'to sign'	firmare

The first column lists Italo-Canadian words, all of which exist in standard Italian as well, together with the English words whose meaning they have acquired. In the second column, the standard Italian equivalents are given. All of the items listed are, in fact, old words which now occur in new meanings and contexts as a result of interlingual identifications. Of the examples given, correre and lavorare are words which have extended their semantic range to include meanings which they do not have in standard Italian; applicazione, fattoria and sciabola, on the other hand, have acquired a completely new meaning and most speakers do not use these terms in their previous meaning at all.

There are also instances, in Italo-Canadian as well as in most language contact situations, in which both morphemic importation and morphemic substitution occur in the same word. A good example is Italo-Canadian gingiarella derived from 'ginger ale' by equating the morpheme 'ale' with the common Italian suffix -ella due to a certain similarity in sound. Other interesting cases of such 'hybrid' forms are patate cippisi, Neapolitan /patánə číp:esə/, from 'potato chips', and /vokéš:ini skuóla/ from 'vocational school'.

English morphemes imported into Italo-Canadian function then as native words do and may take various derivational suffixes. From /bómbo/ (also /bóm:o/) a pejorative /bombáč:o/ and a diminutive /bombičél:o/ are formed; The very common loan /ǰób:a/ 'job' occurs with various diminutive and pejorative suffixes as /ǰob:ičél:a/, /ǰob:ét:a/, Calab. /ǰob:ičéda/, and /ǰob:áč:a/. Similarly, /buk:ét:o/ and /fentsét:a/ or /fentsičél:a/ from /búk:o/ 'book'

and /féntsa/ (also /fénsa/ and /féndza/) 'fence' respectively. The superlative suffix may also be used with borrowed adjectives and the forms /naisís:imo/ from 'nice' and /čip:ís:imo/ from 'cheap' are common.

On the basis of what is known about language contact phenomena, as examplified in the numerous and diverse situations investigated by various scholars, the majority of interlingual identifications likely to apply, in any given instance, in the phonetic assimilation of loanwords can be regarded as theoretically predictable, provided one has available an exact description of the phonemic systems of the two languages in contact, including syllable structure and suprasegmentals. This, of course, obtains in the case of Italo-Canadian.

Canadian English, as spoken in Toronto, may be analyzed as having the following vowel system in stressed position:

iy beat			uw boot
I bit			ʊ bush
ey bait	ʌ rug		ow boat
ɛ bet	ɚ nurse		oy boy
æ bat			
[ɐ car	a pot	ɔ dollar]	(subphonemic)
	ay bind	aw bound	

The assimilation rules which govern the replica of Canadian English vowels in Italo-Canadian are as follows:

1. /iy/ and /I/ > Ital. /i/. Ex.: cheap > /číp:e/; tip > /típ:a/.
2. /ey/, /ɛ/ and /æ/ > Ital. /e/, phonetically realized as open or closed depending on regional pronunciation habits. Ex.: cake > /kék:a/; bet > /bét:a/; match > /méč:o/.
3. /ʌ/ and /ɐ/ > Ital. /a/. Ex.: rug > /rág:a/, car > /kár:o/.
4. /ɚ/, /ow/, [a] and [ɔ] > Ital. /o/, phonetically realized as open or closed depending on regional pronunciation habits. Ex.: nurse > /nórsa/; post > /pósto/, lot > /lót:o/, hall > /óla/.
5. /uw/ and /ʊ/ > Ital. /u/. Ex.: book > /búk:o/; boots > /bútsi/.
6. /ay/, /oy/, /aw/ > Ital. /ai̯/, /oi̯/, /au̯/ respectively, that is they are interpreted as diphthongs. Ex.: size > /sái̯za/ (or /sái̯sa/); boy friend > /bói̯ frénde /; shower > /šáuro/.

As shown by the above rules, the fourteen phonemes of English are interpreted in terms of the five vowels of Italian (the distinction between open and closed e and o is not functional in Italo-Canadian as in many parts of Italy). The rules given are valid, with relatively few exceptions, for all varieties of Italo-Canadian, which differ, however, in the treatment of unstressed vowels. For example, the words /bíl:o/ and /búk:o/ occur not only in this form, but also as /bíl:u/ and /búk:u/, and as /bíl:ə/ and /búk:ə/. The pronunciations with final /u/ and /ə/ are used respectively by Calabrian and Neapolitan speakers, and the difference is straightforward and predictable

(i.e. Italian /-o/ = Calabrian /-u/ = Neapolitan /-ə/). Nonfinal atonic vowels present greater problems and will not be discussed here.

Concerning the assimilation of English consonants, it may be said that different sets of rules apply to different varieties of Italo-Canadian reflecting, in each case, certain structural facts peculiar to a given dialect or group of dialects. For example, the English phoneme /h/ does not occur in Italo-Canadian in general. So, for instance, the words ham, hardware (store), highbeams occur as /ém:a/, /ardué/ or /arduére/, and /aibím:i/. However, the /h/ tends to be preserved by speakers of central and southern Calabrian dialects where an /h/ phoneme is present (chiefly because of the historical change f > h, as in foemina > himmana). Such speakers pronounce the words given with initial /h/, and they tend to retain this sound in internal position as well, as in /zlèǰ:ihámera/ 'sledge hammer', while other Italo-Canadians say /zlèǰ:iámera/.

It is of course not possible to illustrate here all of the assimilation rules which determine the integration of English loans into the various Italian dialects spoken in Canada. I shall restrict myself to mentioning a few interesting examples in which the process of borrowing is seen to conform systematically to structural patterns differing from those of standard Italian.

In the dialect of Campotosto, a mountain village in northern Abruzzi, no post-nasal voiceless stops occur. Thus, when English loans are accepted into this dialect (spoken by about 120 families in Toronto), the following assimilation rule invariably applies:

$$\begin{bmatrix} -\text{ voice} \\ -\text{ continuant} \end{bmatrix} > \begin{bmatrix} +\text{ voice} \end{bmatrix} / \begin{bmatrix} +\text{ nasal} \end{bmatrix} \_\_$$

i.e., a voiceless stop becomes voiced when preceded by a nasal.

Examples are:

a) p > b: /stémbu/ 'postage stamp'
b) t > d: /pendiósa/ 'panty hose'
c) č > ǰ: /béñju/ 'bench', /bóñju/ 'bunch', /iñǰu/ 'inch'
d) k > g: /ǰoŋgu/ 'junk', /téŋga/ 'tank', /tróŋgu/ 'trunk (of a car)'

In Neapolitan, as well as in a number of other dialects, dental s does not occur preconsonantally except before dentals, being always replaced in this position by a palatal /š/. Neapolitan speakers preserve this pronunciation even in the case of English loanwords and the following rule applies:

$$s > \check{s} \ / \ \_\_ \text{ nondental C}$$

Another assimilation rule peculiar to Neapolitan (and other neighboring dialects) is the replacement of English /d/ by /r/. This is perfectly explainable since /d/ does not occur intervocalically or in word initial position in Neapolitan (except as 'double' or long), having been replaced historically by /r/. Examples from Italo-Canadian as spoken by Neapolitans are /arom:áinə/ 'I don't mind', /aroŋgé/ 'I don't care', /miríŋgə/ 'meeting',

/karéǰ:ə/ 'cottage', /šaráp:ə/ 'shut up', /u̯atsəmárə/ 'what's the matter'.

In the dialects of the province of Treviso (Veneto), which are spoken by several thousand people in Toronto, intervocalic l does not normally occur, being usually replaced by a front glide. This is reflected in the variety of Italo-Canadian spoken by Trevisans: for example, the common loanword sciabola 'shovel' is pronounced by them as /sáboe̯a/. Calabrians, on the other hand, pronounce this same word as /sáb:ola/ with a long /b/ because in the dialects of Calabria (as well as in the Calabrian pronunciation of standard Italian) intervocalic b occurs only as long.

Thus, as the examples given above indicate, in order to account precisely for the phonetic shape of any given Italo-Canadian word of borrowed origin, a knowledge of the phonemic system of the dialect in which it is found is required. An in-depth study of Italo-Canadian would thus have to be based on accurate descriptions of the sound systems of a considerable number of Italian dialects. Clearly, given the well known range of Italian dialect variation, this would be a monumental task.

The Italo-Canadian corpus on which this paper is based contains also, however, a number of loanwords whose actual shape would not appear to be theoretically predictable on the basis of a contrastive description alone. It would be interesting to discuss these, but time prevents me from doing so today. Regrettable though this may be, one is reminded of Voltaire's dictum: "le secret d'ennuyer c'est de tout dire". Let me therefore follow his advice and stop here.

ENGLISH HUNGARIAN AND HUNGARIAN ENGLISH LANGUAGE INTERFERENCE PHENOMENA IN CHICAGO

Izabella Horváth Janda
University of Illinois at Chicago Circle

The purpose of this pilot study was to shed light upon the kinds and frequencies of interferences Hungarian and English exert upon each other in the speech of first generation bilingual Hungarians currently residing in Chicago, and the social and economical factors (acting as motivations) influencing these interferences. The ages of the informants ranged between fifty-five to sixty and they acquired the knowledge of English between the ages of thirty to thirty-five.

It was postulated that the phenomenon of interference of these languages are in parallel existence, yet actual interferences of English upon Hungarian are not the same (as to levels), as is the interference of Hungarian upon English. The most observable interferences from Hungarian into English in the speech of a bilingual is on the phonological level, $^{H}/v/$ instead of $^{E}/w/$, the rolling of /r/ instead of the use of retroflexion are some of the many examples . However, the very same speaker speaking Hungarian would exhibit the most interferences on the lexical level, that is borrowing English vocabulary items and treating them grammatically the same way as any Hungarian word.

All of the informants shared this generally uneven interference pattern. Factors causing such unevenness are several. I wish to mention only three. First, the fact that there is great interference on the phonological level can be traced back to the informant's relatively late age at which he acquired his knowledge of English. This part of language, as pointed out by Weinreich (1953, p.76) will always show some degree of interference from the native tongue. The second factor, concerning the high frequency of English loan words in Hungarian speech is twofold: some English words do not have exact Hungarian equivalents; "smog", "teenager", and "coffee-break" are examples. Other English words are frequently used in daily English discourse and by this frequent verbal encounter they become an integral part of spoken every-day Hungarian speech. Examples of these are terms such as "income tax","car", "elevator", "to drive", just to mention a few.

While the above mentioned pattern seemed to be constant, what varied was the frequency with which these interferences occured on each level. Thus it was suspected that the fluctuations of these interferences were

influenced by social, economic and psychological factors.

Examining the interferences from a social point of view, we will find that the bilingual who considered the acquisition of the English language very important because he wished to be a member of the society (that is to begin to learn new behaviour patterns as well as new linguistic patterns), achieved a more native-like competence in English than the speaker who did not share the feeling or had the same social drive.

Such a speaker, who being in Rome, wanted to do as the Romans do, usually immersed himself into the English speaking community. He began to socialize with English speakers only, isolating himself from the speakers of Hungarian. In time his linguistic performance in Hungarian showed great interferences from English mostly on the lexical and sometimes at the syntactic levels, although never on the level of phonology. He would attach great social importance to his ability of using borrowed English vocabulary items in his Hungarian speech. This type of linguistic behaviour showed how well-adjusted he really was in the new society. Expressions such as the following, although not widespread, are not uncommon: / ríngoltom ɔ béllodɔt de nem énseroltal/, "I rang your bell (doorbell) but you did not answer."

Conversely, we can find the person who doesn't share this enthusiasm for the acquisition of English. He does not feel the need for a new group identity. His sense of belonging is provided him by a small but constantly functioning group of compatriots, who socialize together and speak Hungarian on a regular basis. This person, isolating himself from the English speaking community is a displaced person in the true sense of the word. He is an individual disappointed in what life in the United States can offer him. Most often he was a white-collar worker in the old country who enjoyed some prestige, such as a lawyer, teacher, government official or the descendant of an aristocrat. Having come to the United States, he suddenly finds himself being a manual laborer, a position that greatly diminishes his prestige in the eyes of other Hungarians as well as himself. Adhering to the social value system of the old country, his resentment toward the new society and his undesirable role in it results in the stubbornness of refusing to learn English, never suspecting (or not even caring) that he is hurting no one but himself. He manages to learn only the rudiments of English, which enable him to obtain the essentials of daily life. He does not want to improve his English pronunciation or to correct any grammatical mistakes in his English speech, thereby allowing himself to speak in a manner that to a linguist spells heavy Hungarian interfernece on these levels.

The levels of semantics suffer no less; often this speaker does not (or refuses to) acknowledge the fact that

not every Hungarian expression can be translated into English and have the expressions elicit the same reaction from an English speaker as that from a Hungarian speaker. He often makes direct translations from Hungarian to English leaving his audience at best confused. Not receiving the expected response from the listeners, he concludes that the reason for their lack of understanding is that they (his audience) are of "proletariate descent", "uneducated and uncultured", unlike he who is a descendant of "cultured" ancestors, and is therefore "above" them in "intelligence".

Let's examine this person's competence in Hungarian. He frequents places where a great number of Hungarians aggregate, and if he was a person of social prestige and high status in Hungary, here he enjoys this same prestige, although to somewhat lesser magnitude. His Hungarian speech is usually unmarked by interferences from English on all but the lexical level. (however, he tries to maintain the only tangible object of his past social status--his language--which, if spoken well, gives him added status. Because of this he tries to borrow as few English vocabulary items as possible.

The educational level of the speakers is another social variable. An individual, whose schooling did not exceed more than a few years of grammar school, finds the learning of English more difficult. His linguistic attempts on the phonological and syntactic levels will often be frustrated. He seems to lack the analytical approach to learning in general and his attempts at learning a language confuses him. He proceeds to memorize each new word instead of applying rules that might be helpful in learning. He, too, applying old social values to syntax and semantics, makes the mistake of verbatim translations from Hungarian to English. Direct translations of Hungarian jokes are classical examples of this type of interference.

He may be quite competent in his Hungarian speech for the very reason that he is so incompetent in learning English--not much interference can be expected from a language one does not know how to use. However, the very feeling of being a "failure" in his English language learning, he tries to compensate for this by borrowing heavily from the English lexicon, affixing each item as if it was a Hungarian word. The word "car" in an Hungarian context will be affixed with the accusative /t/ ending, becoming /kart/, or it may gain an ablative case ending /-bol/ resulting in /karbol/(=from the car).

Thus, we can see that a relatively uneducated person has much interfernece from Hungarian in his English speech but there is only lexical interference from English. A few persons are so inapt (if I may use this term here), at second language acquisition (not communication!!), that

the two languages "flow together" into a mixture of Hungarian and English. (The exact pattern is not yet clear as to levels.) They use this form of speech with occasional adjustment, to both speakers of English and Hungarian. At this point, it seems that with this method the only way (or at least oneof the ways) they can make themselves be understood is by alternate repetition of key or function words in both languages within the same sentence. An example: English sentence, "I go to the drugstore.", Hungarian sentence "A drogériába megyek."— These concepts will be expressed by this particular speaker in the following manner: /ay meǧek go drugsturho/. (Here the verb go=megyek is repeated in both languages, the speaker seems to realize the importance of the word in the sentence.)

Now let's look at the affect of economic factors on interference. It seems that a speaker, who was forced to learn the language because his economic well-being (or advancement) depended on it, has less Hungarian interference in his English speech. His phonology may not be as good as that of other speakers, whose morphology and syntax have more interference, but it is on these levels that he has more competence. Usually, however, along with the other levels, his phonology tends to be better than that of those speakers who demonstrate a definite resistance to producing correct English pronunciation. This can be noticed in his speech: sometimes he manages the production of /w/ and the retroflexed /r/, sounds both of which do not exist in Hungarian.

This speaker doesn't exhibit any interference from English to Hungarian on the phonological level, but his lexical borrowings from English are notable in his Hungarian speech. His major concern is not to place value judgments upon either language, but to communicate in the most expedient manner with whatever linguistic tool he has at hand. His reasons for borrowing English vocabulary items into Hungarian are different from those individuals discussed previously.

Another speaker, who did not need to learn English because economic conditions did not force him to do so, (housewife, or person earning a living by handwork only) also showed noticeable interferences from Hungarian on all levels of English discourse. For example, a housewife, caring for her bilingual children, exposed to the daily affects of television, had ample time and opportunity to learn English, but we find that this is usually not the case. Her economic security was insured by a working husband and many of the daily chores of shopping, banking or occasional telephone calls by sales people were to be her only duties. These chores, however were handled by her children, who became the natural (oftentimes all too

reluctant)go-betweens for the parent and the English speaking outside world. This widely used method worked much better than we might suspect, and the parent, using various psychological stratagems to arouse the conscience of the children, was thereby saved from the "agonies" of having to speak English for long periods of time.

This person's competence in Hungarian is quite unaffected by English on the syntactic and pholological levels, but lexical interference again manifests itself strongly, in much the same way as discussed above.

It seems, therefore, that socio-economic factors do seem to influence the magnitudes of interferences on both languages, eventhough the interference of English on Hungarian is not the same as that of Hungarian upon English at any given time in the speech of any given person.

It is possible therefore, to find speakers of the first generation bilinguals, who are well educated, but having an attachment to Hungarian resist the acquisition of English and this strong opposition will affect his native language in the reverse order--that is--he will have generally less interference from English on all levels.

Similarly, it is equally possible to meet a relatively uneducated person with a great desire to learn the second language because of his need for social communication and new group identity within the new society. This speaker will acquire English speaking knowledge with greater speed and use it with less Hungarian interference. His Hungarian speech may show interferneces because of his improved English competence, but if he is emotionally attached to his native tongue his performance will be relatively free from English interference.

In all of the instances discussed the underlying current of psychological variables can be clearly seen. Likes, dislikes, fears, hopes all play a role in language learning and interference and, needless to say, complicate the issue even further.

Thus, the factors, such as the age of second language acquisition or the frequency with which any one language is spoken, are not the only ones that affect the degrees of interferences in the speech habits of an individual capable of using both languages. Psychological, social and economic variables also influence this linguistic phenomenon. The amount of interference on each level and of each language upon the other changes according to how many variables apply to the speaker.

It would be fruitful to examine the problem more accurately to see if any regularity or pattern could be found. At this point, it is impossible to say more about the frequencies of interferences, even as to types within any given level, (such as which are the exact phonemes mispronounced by a particular speaker, on what occasions

and why). If such a method of measurement could be devised, or an already existing method utilized we may hope for more accurate conclusions.

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BILINGUAL PHONOLOGY: SYSTEMATIC OR AUTONOMOUS?

Valerie Becker Makkai
University of Illinois at Chicago Circle

In looking at the literature on bilingualism, it appears that the term *bilingual* has been used in different ways by different people. Thus, we must define our terms before we begin. If we take the word in its strictest sense, meaning an individual who speaks two or more languages with true native fluency, then presumably the two languages are kept entirely distinct by the speaker, there is no interference between the languages, the phonology of each is flawlessly reproduced, and we have nothing further to say on the topic. This topic could still be salvaged, however, if we put that bilingual individual into the setting of an entire speech community of bilinguals, all or many of whom control the same two languages with true native fluency. Because then our *measure* of native fluency can be different. An isolated bilingual individual must reproduce faithfully the phonology (and of course all the rest of the grammar) of both languages to the satisfaction of *non*-bilingual speakers of each language. The slightest distortion is picked up by non-bilingual speakers as a "foreign accent". On the other hand, in a bilingual speech community the individual is judged, not by the standards of non-bilingual speakers, but rather by the standards set up by the community of bilinguals. And these standards, in-so-far as they differ from the standards of the language as spoken by monolinguals, can be said to constitute a new dialect of that language. The distortions caused by interference between the two languages are no longer regarded as a "foreign accent", but simply as evidence of dialectal differentiation caused by the contact between the languages.

Much has been written about languages in contact and it is a fascinating topic, both sociologically and linguistically. But I am still concerned about the isolated individual who makes the same distortions that might occur in a bilingual speech community and who yet, because he is not part of such a bilingual community, is simply labeled as having a foreign accent. According to our strict definition above he would not be a true bilingual, yet his problems are very real, and are no less interesting in the study of bilingualism than are those of an entire speech community. In fact, a study of the almost-but-not-quite bilingual individual may serve to shed light on the processes at work in language contact. Thus I shall confine my remarks here to a discussion of the phonology of such semi-bilingual individuals.

The phenomenon of interference has, of course, been widely discussed and debated. In some early treatments of the topic it was implied, if not explicitly claimed, that most if not all errors in second language learning could be attributed to interference from the native language. Later writers on the subject quite correctly recognized that many other factors can also produce errors, one of the most prevalent of these being imperfect learning of the overall system of the second language. Thus overgeneralization of a regular pattern such as *one cat–two cats, one book–two books* can easily lead to forms such as *one foot–two foots* or *one mouse–two mouses.* A similar failure to learn the exceptions to the rules results in forms such as *I do–he does* /duwz/ or *I say–he says* /seyz/–*they said* /seyd/. Failure to learn the patterns of distribution of the /t–d–id/ past tense marker in English can result in forms such as *walk–walk-ed* /wɔked/ or *hope–hope-ed* /howped/ even when the phonological rules of the native language would not interfere with a final /kt/ or /pt/ cluster. Such imperfect learning of the system of the target lan-

guage results in distortions based not on interference of the native language but on *the forms of the target language itself.* Thus it appears that along with native language interference we can also list *target language interference with itself* as a strong factor in language learning errors.

The question now arises, what, if any, are the implications of such double interference for the theory of bilingualism and for linguistic theory in general? Can current linguistic theories shed any light on, or be of use in solving, the interference problems of a bilingual? And specifically, does systematic phonemics have anything to offer which autonomous phonemics does not?

Traditionally in contrasting two languages, it has always been considered sufficient to make a complete autonomous phonemic description of each of the languages and to compare points of agreement and disagreement between the two systems. Thus if the target language has a /θ/ phoneme whereas the native language does not, it can be predicted that the /θ/ will present problems for the learner and very likely be mispronounced as either /t/ or /s/. If the target language has two phonemes /b/ and /v/ whereas the native language has both these sounds but as allophones of the same phoneme, it can be predicted that in the target language [b] and [v] may be distributed by the learner exactly as they are in his native language, resulting in incorrectly substituting [b] for [v] and also [v] for [b] in certain environments. If the target language allows initial /st-/ clusters while the native language does not, then this cluster will present problems even when the native language has the phonemes /s/ and /t/. Thus we see that phonological interference can arise when there is lack of agreement between the two languages on any of several levels—presence vs. absence of whole phonemes, presence vs. absence of individual allophones of phonemes, distribution of allophones, distribution of whole phonemes, or any combination of these.

The above type of autonomous phonemic analysis has been used with considerable success in predicting and analyzing problems of interference between two languages. Obviously only a theory which recognizes phonemes and allophones as independent units can make use of such a direct approach. A theory such as transformational grammar, in which phonology is considered as an extention or continuation of a more central syntax or semantics must approach problems of phonological interference in an entirely different perspective—namely, as part of the total grammatical systems of the languages involved. As a result, information about the phonology of the language is distributed throughout long sequences of morphophonemic rules, and nowhere is there a concise statement of the generalizations which can be made from these rules—that is, there is no place where the phonemes and their allophones and their distributions are listed, because, of course, there are no such things as phonemes and allophones. (It should be noted, of course, that a few transformational linguists, starting with Johns in 1968, have been arguing for reinstating the phoneme but without widespread acceptance.) The consequence of the traditional transformational approach for a study of language interference is that I must plow through a lot of irrelevant and immaterial details before I can extract the information that I need about the particular instance of interference that I am interested in. If the problem is one involving whole phonemes, such as the inability to make the English /iy/–/i/ distinction, then it matters little whether the /i/ that one has trouble with is derived from an underlying /ay/ as in *decide–decision,* or is itself an underlying form, as in *with* or *give*. It is not the source of the /i/ that gives trouble but rather its very existence, however it got there. I do not need to know a single morphophonemic rule, let alone syntactic rule, to deal with such an interference problem. All I need is to know the phonemes of the languages in question and how they match up, or fail to match up, with each other.

It is quite unfortunate that the majority of transformational grammarians have rejected the concept of the phoneme and its allophones. These are valuable generalizations which we can ill afford to dispense with even in a monolingual study, and especially

so in studies of bilingualism. The ability to treat the phonology of languages as autonomous systems is essential, I feel, to a study of inter-language interference. The autonomous nature of the inter-language interference problem can perhaps best be illustrated by the fact that many semi-bilinguals are able to learn the syntax and semantics of their second language to such perfection that they would be taken for true native speakers if it were not for their imperfect mastery of the phonology. Think of foreign speakers of English such as Arthur Koestler, Vladimir Nabokov, or Jacques Barzun who have mastered the English language to such an extent that they have become well-known English writers –yet the moment they open their mouths to speak they betray their non-native origins. Clearly, then, phonology must, at least in some respects, be autonomous from the rest of language–if it were not autonomous then the very fact that an individual has perfected the syntax of a given language should automatically result in perfect phonology, and imperfect mastery of the phonology would have to imply imperfect syntax as well.

I do not mean to imply that a systematic approach to phonology is not, under certain circumstances, very useful as a theory and even as a practical teaching device. Rather, my claim is that in actual point of fact the speaker's phonological performance is quite separate and distinct from his semantic and syntactic performance. The question of how these performances relate to his competence is another matter which will be taken up below.

So far I have spoken only of inter-language interference–that is interference of the two languages with each other. But what about intra-language interference–the sort of interference produced by underlearning or overgeneralizing the rules of the target language itself? It is here, I feel, that a systematic approach to phonology has the most to offer, precisely because it, unlike autonomous phonemics, does treat the phonology as part of a larger overall system. Intra-language phonological interference problems rarely occur on an isolated phonological level, but rather usually occur on the morphophonemic level, involving imperfectly learned mophophonemic alternations within the target language.

Those strictly phonological problems which appear to be of the intra-language variety can usually be shown to derive ultimately from native language interference. Thus a German who has learned to pronounce English /w/ perfectly, even though his native language has no /w/, may continue to mispronounce English words, confusing /w/ with /v/ and vice versa. Thus, a well-known German linguist who lived in this country for decades, continued to pronounce *word* as *vord,* but he also pronounced *verb* as *werb* and *vowel* as *wowel* (fortunately not **wovel*). His problem appears to be simply overlearning of the /w/ so that he uses it even where he ought not to; but, of course, the ultimate cause of the overlearning is still native language interference–the basic uncertainty as to which sound is which. A speaker of a language where /w/ and /v/ both occur as separate phonemes would never have the initial problem of learning to distinguish the sounds, and thus, since he has nothing to learn, he could not possibly overlearn it. Phonological overlearning, then, appears to occur specifically in those areas where there are no native language parallels to the target language form, or where the native and target language forms are distinctly different from each other. Thus this type of overlearning is always ultimately caused by native language interference.

But on the morphophonemic level, as mentioned above, the errors are frequently quite unrelated to any native language patterns, and are based instead on the imperfect learning of target language morphophonemic alternation patterns. Thus 'he /duwz/' and 'he /seyz/' result from not having learned to make the proper morphophonemic transformations, or, in structuralist terminology, from not having properly learned the allomorphs of the *do* and *say* morphemes. The imperative uses of *have* in *have to* and *has to* are frequently mispronounced by non-natives as *have* /v/ *to* and *has* /z/ *to*, with /v/ and

/z/ rather than /f/ and /s/ because they fail to distinguish this function of the verb as one requiring different phonemic realizations from the usual ones. Mistakes such as *decide–decision* with /ay/, and *deride–derision* with /ay/, or *vain–vainity, Spain–Spainish, nation–national,* all with /ey/, could again be corrected by proper learning of the morphophonemic alternations at work in these, and large numbers of other, English words. An approach which recognizes the systematic nature of these alternations and capitalizes on them in language teaching situations will have much better success than one which treats each such alternation as an isolated occurrence.

American structuralists have, in theory if not in practice, had to treat such irregularities as isolated phenomena since they never formally recognized the morphophoneme as a unit, nor a morphophonemic level of analysis as distinct from the morphemic. Thus *vain, Spain* and *nation* are separate morphemes, and the fact that they all partake in derived forms in which the /ey/ is realized as /æ/ cannot be overtly reflected in the morphemic or phonemic analysis. Many structuralists get around this problem by creating lists of forms which behave similarly. *Vain, Spain,* and *nation* would, then, be cited, along with others, in the list of those forms which display the /ey/–/æ/ alternation. This kind of listing may well have considerable value in a language learning situation, but it has the disadvantage that it does not demonstrate the way in which such alternations fit into the total system of the language. Systematic phonemics, *à la* Chomsky, does integrate everything into a systematic whole, but somehow, even on the morphophonemic level, one is always left with the feeling that one can't see the woods for the trees. The major, more generalized patterns do not emerge clearly for the language learner because the path is strewn with so many minor rules created to deal with the exceptions to the generalities. Perhaps in a language learning situation, at least, taxonomic listing of such things as phonemes and allophones, or irregular verbs and nouns, or whatever, is not such a bad thing after all. At least the learner has clearly outlined before him a list of those forms and irregularities and exceptions to the rule which he must master in order to approach native proficiency in the language.

Let us go back now to consider the almost-bilingual individual. He has already mastered, in one way or another, most of the features of his second language. He has the syntax and semantics pretty well down pat, and the irregular morphophonemic alternations that originally gave him trouble (just as they trouble most children in native language acquisition) have been fairly well ironed out. But on the purely phonological level he still occasionally makes an error—not often and not even necessarily predictably—but nevertheless sooner or later he betrays himself to a careful listener as a non-native. Why is this? Is there some process at work here, an investigation of which might help such an individual to do away with his accent?

The answer appears to go back to the competence-performance dichotomy. In a monolingual native speaker of a language, the distinction between competence and performance may be somewhat academic—a native speaker may not ever perform all of his competence, but he never violates his competence (except perhaps in special cases such as extreme fatigue or distraction). That is, he never performs something which goes against the rules of his competence grammar. And this is exactly where a non-native differs from a native—because an almost-native speaker may in fact have all the competence of the native—that is he knows all the rules and even knows how to perform all the rules—but he can, and in fact often does, violate the rules. The reason this is so is that, in contrast to the monolingual individual, the bilingual carries around in his head two separate and complete competence grammars—one for each of his two languages. These are normally kept quite distinct from each other, and as those readers who are fluent speakers of more than one language well know, one can switch back and forth between these competences much as one switches channels on the TV. But what apparently hap-

pens is that occasionally a short circuit occurs between the two competences which results in an impure performance—static, if you will, caused by the neighboring channel. This short circuiting appears to happen most readily on the phonological level, less so on the syntactic, and fairly rarely on the semantic. In fact, on the phonological level, depending on the individual, the shorting of certain circuits may be so consistent that it becomes the established pattern, entirely by-passing the correct circuitry. Thus, in the above example of the German who confused /w/ and /v/, the shorting became so severe that he never did sort out which was which in his performance, even though on an intellectual level (that is, in his competence) he certainly knew the difference and knew where each sound belonged. Sometimes the short circuiting occurs only sporadically, such as when the speaker is tired or has just been speaking his other language, or when the phonological environment is such that it presents special difficulties. Thus many non-natives can pronounce correctly a word like *south*, but put the /θ/ in the context of a lot of sibilants and /t/s, as in *South East Asia,* and the phrase may well come out as *Souse East Asia* or *Sout East Asia.*

If we are to be able to help the almost-bilingual person we must determine the places where such short circuiting occurs or is likely to occur and then we must endeavor to insulate those circuits from each other in some way. The first task is easy—the second is probably impossible, but that does not mean we shouldn't try. It is easy enough to determine where shorting will occur because it is, in fact, as we saw above, almost always at those points where the native and second language systems are at variance with each other. It is as if, at those points, the speaker, instead of constructing the entirely new circuitry that he would need to handle the form of the second language, tries to make do with the already existing circuitry of his native language, with perhaps some minor modifications. This generally results in disaster.

For example a Hungarian learning English constructs for himself a sort of interlanguage super-morphophoneme whose members are /θ/ and /t/. In speaking Hungarian he activates /t/ in the proper places, but, of course, never /θ/, since Hungarian doesn't need it. Now when he gets to English, again he uses /t/ where it ought to be used, since for the most part the distribution in English is similar to that of Hungarian. But when it comes to /θ/ he has to make an effort to produce it correctly, since this is a new sound for him. Furthermore, he is never quite sure of the proper distribution of /θ/, but since he has lumped /θ/ and /t/ together into the same super-morphophoneme he feels that basically any one of the "allo-morphophones" ought to do, and so whenever /θ/ is inconvenient he allows /t/ to replace it. He has, in fact, constructed a rule in his competence grammar of English which ought not to be there. The problem is that once something gets into ones' competence grammar it is awfully hard to get rid of it. This is true for native speakers as well as non-natives—think of the thousands of hours English teachers spend trying to "correct" their students' English, with really very little result. It is easy enough to generate and delete things on paper, but when it comes to the reality of linguistics—and I, for one, still happen to regard linguistic reality as coming in human form rather than in abstract formulas—when it comes to the reality of the matter, generating a new rule or deleting an improper one is a very difficult process. But this is what we must help the almost-bilingual to do if we are to help him at all. Old habits die hard, but if we are persistent we may meet with some small measure of success.

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VIII

LINGUISTICS AND THE ARTS

OLD ENGLISH PROSODY AND DESCRIPTIVE LINGUISTICS

Richard Tow
State University College at Buffalo

The purpose of this paper is to present a theory of Old English prosody, a theory that, hopefully, will both do justice to the intricacies and the beauty of the verse and will, at the same time, be true to the nature of the English language before the Norman Conquest. The basic formulation of this theory was developed by Henry Lee Smith, Jr. and his students during the years preceding Dr. Smith's death in December, 1972, but the theory has never been formally presented. I submit this paper as one among many who worked with Dr. Smith in refining his ideas about Old English poetry and testing his theory against other descriptions of that verse form.

Several descriptions of Old English verse have been attempted, and, therefore, at least a short preliminary discussion of them is necessary. To begin with, the "strongest" theory of Old English prosody, strongest because it has stood the test of time and has provided the basis for nearly all subsequent theories, is the so-called "five type" theory proposed by Edouard Sievers in the late nineteenth century. It was known before Sievers' study that Old English poetry consists of verses divided into half-lines, and that at least one but usually two words in the first half-line of a verse alliterates with at least one word in the second half-line. In addition to this, each half-line nearly always contains two primary-stressed syllables, regardless of whether both such syllables alliterate with syllables in the other half-line. Sievers discovered that there are five types of half-lines that could be identified according to the configurations of stressed and unstressed syllables. The most frequently occurring half-line, type A, for example, contains a strongly-stressed syllable followed by one or more weakly stressed syllables followed by another strongly stressed syllable, followed again by a single weaker syllable. Type B, the next most frequent variety, reverses this pattern; and type C lines are those having a weak-strong-strong-weak sequence. Types D and E are other variations on this basic pattern of weaker and stronger stresses. Extra syllables often occur at the beginnings of half-lines as "anacrusis," and medium-stressed syllables sometimes combine with other syllables or are "resolved" in order to take the place of a missing strongly stressed syllable. The first example below is a type A half-line, the second is a type B, and the third is type C:

A. þrým gefrúnon

B. he þaes frofre gebad

C. Oft Scyld Scefing

One weakness of Sievers' theory lies in the necessity for the many rules covering various exceptions to regularity. The type B line above, for example, is a modification of the strict weak-strong-weak-strong pattern in that it contains one extra syllable (he) preceding the first strongly stressed one. It also has two weakly stressed syllables between the strongly stressed ones, making it necessary, according to Sievers, to "resolve" the two syllabes of "frofre." This process of "resolution," while it has the advantage of making lines conform to a metrical design, is linguistically questionable. Since all syllables in English must, by their very nature as syllables, have some degree of stress, it would seem more reasonable to admit that in type B lines, for instance, more than one weakly stressed syllable may occur between the strongly stressed ones, a practice which Sievers' theory forbids.

In an attempt to resolve some of the problems of complexity and necessity for "exceptional" rules in Sievers, an interesting theory of Old English prosody has recently been introduced by Samuel J. Keyser.[1] This theory has won acceptance by many linguists and students of poetry, and indeed it has much to recommend it, especially in its ability to describe simply and economically the basic OE verse design. Keyser's proposal consists of two parts. First, there are "abstract metrical pattern rules" governing the general shape of all lines in Old English verse:

1. Verse = half-line + half-line
2. Half-line = XS(S) [2]

A set of "realization rules' describes the allowable ways in which the abstract pattern is actualized in each given line of verse.

The revolutionary element in Keyser's theory is his suggestion that a half-line need contain only one strong-stressed syllable and still be considered metrical. This makes it possible for Keyser to ignore the question of "resolved" stresses. The difficulty caused by the occurrence of extra syllables between the two strongly stressed syllables of a half-line is neatly handled by the first of Keyser's "realization rules":

> An S position is actualized by a subsequence beginning with a syllable containing a fully stressed vowel followed by any number of syllables containing vowels of lesser stress. [3]

Keyser's theory, in sum, argues for a single "verse design," to use Roman Jakobson's term, that is realized in various ways in each "verse instance," or each separate line, of Old English poetry. In my presentation of the Smith theory, however, I will try to show that Keyser's description of this verse design is perhaps too simple to provide an accurate idea of the way the poetry works.

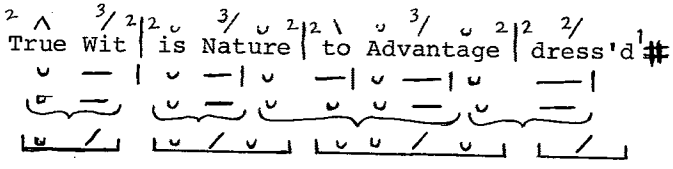
The description developed by Smith, to begin with, differs in one remarkable way from both Sievers' and Keyser's theories. According to Smith, the verse design rules of OE poetry are as follows:

1. There must be at least four primary stressed syllables per verse, that is, at least two primary stressed syllables per half-line.

2. There must be at least one syllable per half-line which bears at least pitch /3/ and which alliterates with at least one other such syllable in the opposite half-line.

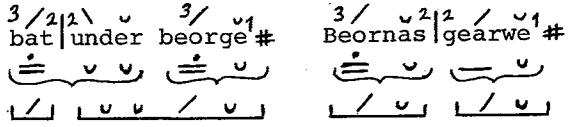
3. There may be no more than eight syllables per half-line.

The first of these rules requires little explanation. It recognizes that the basic shape of OE verse is a four-beat pattern. Sievers, Keyser, and nearly all other students of Old English verse can agree with this. Smith's description differs from others, however, in suggesting that a half-line can be considered metrical even if it contains more than two primary stresses. Later, I will show why this suggestion is both linguistically and metrically feasible. It is first necessary, however, to discuss the implications of the second of Smith's rules. The reference to pitch in Smith's rules of verse design separates his description from all the traditional theories of OE prosody. This acknowledgement of pitch as a structural feature of OE verse, however, derives directly from a three level method of analysis used by Smith and his students in dealing with other English verse forms.[4] The rhythms and cadences of accentual-syllabic verse arise from the interplay among the three systems of suprasegmental features of English: stress, pitch, and juncture. A typical three level scansion looks like this:



The first level of scansion, the meter level, simply records the relative strengths of stresses regardless of their relationships to the four-level stress system of English. This level provides the basis for nearly all poetry in English outside vers libre and native accentual forms. The second line describes the rising or falling natures of the pitch contours of the line. In the example given, all of these so-called "measures" are rising. The third line, the "cadence" level, records the syllabic lengths of intonation patterns, or stretches of material bounded by terminal junctures and containing one primary stressed syllable. Interplay among the levels arises first, from the variety of ways in which the metrically stronger and weaker stresses are actualized by four levels of English stress, and second, from the rising and/or falling of pitch, which has the ability to override terminal junctures.

Smith's second rule for Old English verse, recognizing the formative nature of pitch, suggests, furthermore, that in this verse alliterating syllables were given emphasis by the singers or reciters not merely in their being primary stressed but by carrying a semological signal of emphasis, namely higher pitch. The scansion of a line of OE verse takes this form (We can, of course, omit the "meter" level of scansion because the verse is purely accentual, not accentual-syllabic):



This line is a fairly "normal" one, having the characteristic falling rhythm of OE poetry. In terms of notation, occurrences of pitch /3/ are marked within measures by a double line, pitch /2/'s by a single line. The word "bat" is the arsis of the first measure and the first syllable of "beorge" the arsis of the second. A small amount of complexity lies in the fact that the first measure of the first half-line extends beyond the first terminal juncture and thus incorporates the first two syllables of the second cadence foot. The second measure begins with the occurrence of pitch /3/ on "beorge," even though two syllables precede "beorge" in the second intonation pattern.

I want to return now to Keyser's theory and compare his handling of some apparently "unmetrical" lines to ways in which these same lines can be dealt with and treated as metrical using the Smith method. An alternative Keyser's first Realization Rule states that "the second S in each half-line may extend beyond the first to the second final word boundary." [5] In order for the second S of a half-line to extend to the second final word boundary, no syllable in this second word can take pri-

mary stress; that is, the maximum number of primary stresses per half-line is two. This, of course, differs from the statement in the Smith theory that there can be more than two primary stressed syllables per half-line. Keyser gives the following examples of half-lines which apparently contradict his rule:

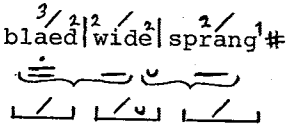
blaed wide sprang

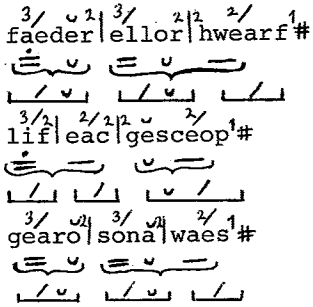
faeder ellor hwearf

Keyser admits that the nouns, adverbs, and verbs in these lines all would normally carry "major stress," and the lines might, as a consequence, be considered unmetrical. But one way that the lines can be considered metrical, according to Keyser, is if we assume "that stress subordination has taken place."[6] With reference to the half-lines in question, Keyser states that "we may suppose that the pre-positioning of the adverb provides it with a primary stress while simultaneously subordinating the stress of the following verb."[7]

Keyser's explanation of stress subordination is unconvincing with regard to linguistic facts. If the normal order in Old English was indeed verb followed by adverb, the inversion of this normal order would, as Keyser says, impose primary stress on the adverb (which, quite possibly, would have primary stress anyway); but it is not at all clear why such an inversion would subordinate the stress on the finite verb. Inversion might just as well insure the placement of a terminal juncture between adverb and verb; and if the finite verb is followed by another terminal juncture, as it is in the examples, there must be primary stress on the verb, since it comprises an entire intonation pattern.

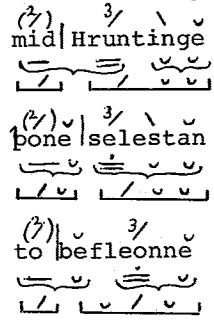
There are many cases of apparently "too heavy" lines in OE verse, and I believe that the Smith theory provides a more reasonable explanation of how nearly all such half-lines preserve the basic two-beat rhythm while containing more than two primary stresses. Here are the previously quoted half-lines and other examples of heavy lines that Keyser cites, as they can be described in a two-level scansion:





All of these lines have but two measures and thus only two more prominent "beats." The other primary stresses are subordinated by pitch, not stress. Thus the lines are not only metrical but "regular," although the two level system of analysis also clearly shows their more complex actualization of the verse design.

The other major difference between the Keyser and Smith theories has to do with apparently "light" half-lines, ones in which there is apparently only one primary stress. In terms of Keyser's theory, of course, all of these lines are metrical, since only one primary-stressed syllable is all that is necessary per half-line. Keyser gives the following examples of what he considers "one-stress" half-lines. I have provided a suggested two level scansion below each example:



In order to be considered metrical in terms of the theory proposed here, the words "mid," "þone," and "to," which do not normally bear more than medial stress, must carry primary stress, as I have shown in my scansions of of the lines. Keyser, however, argues against the imposition of what he calls "emphatic stress for the sake of the meter," claiming that such a practice "destroys the very distinction which a theory of prosody is designed to illuminate, namely, the difference between a metrical and

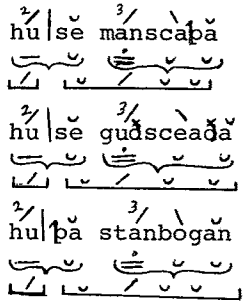
an unmetrical line."[8] To this objection it may be answered, first, that imposition of primary stress does not seem to have much (if any) effect upon the meaning of these lines as long as the pitch on the words in question remains at a "neutral" pitch /2/. It also seems to me that Keyser overlooks the effects of an established rhythm in the poetry, a rhythm that sometimes has the power to elevate stress without actually breaking the rules of language.

Two other examples given by Keyser, however, suggest another reason why many of the light half-lines in OE can be treated as metrical according to the Smith theory:

hu þa aeþelingas

hu hit Hring-Dene

The word "hu," at the beginning of these lines, according to Keyser, cannot bear primary stress, and, therefore, the lines must be considered one-stress lines. But in Beowulf "hu" occurs at the beginnings of half-lines frequently enough (twelve times) for us to consider the possibility that such constructions constitute a type of poetic formula that serves both a narrative and metrical purpose. In the following lines, not cited by Keyser, "hu" occurs at the beginnings of "light" half-lines apparently for the purpose of taking a primary stress and thus "filling out" the two-beat pattern:



It also seems probable that the numerous "light" half-lines in Beowulf beginning with finite forms of "to be" (waes, is, etc.) point to the poet's use of yet another such metrical "formula."

I believe that we can draw two general conclusions from this comparison of the theories proposed by Keyser and Smith. While Keyser's theory is a highly economical one in its statement that metrical half-lines may include only one primary-stressed syllable, it seems to disregard

the overwhelming evidence that the verse design of OE poetry is a four stress pattern. The Smith theory, in acknowledging the four stress design, points to various ways in which the Old English poet, working with his spoken language in the context of a strong verse tradition, could realize the abstract design in actual lines of poetry.

The second conclusion that can be drawn from our discussion is that the Smith theory, in proposing that pitch played an important role in the structuring of OE verse, allows us not only to resolve many questions concerning apparent metrical irregularity but also to appreciate more fully the intricacies of OE verse. The use of a two-level method of prosodic analysis, in other words, helps us to understand both the restrictions as well as the freedom of the Old English poet in using language as the material of art.

Notes

1. Samuel J. Keyser, "Old English Prosody." College English, XXX (Feb., 1969), 331-356.

2. Keyser, 333.

3. Keyser, 333.

4. See Elizabeth K. Hewitt's "Structure and Meaning in T. S. Eliot's Ash Wednesday." Anglia, LXXXIII (1965), 426-450.

5. Keyser, 333.

6. Keyser, 337.

7. Keyser, 337-338.

8. Keyser, 336.

CONTEXTUALIZATION AND THE CINEMA

Richard J. Leskosky
University of Illinois at Urbana-Champaign

"The language of the cinema" and "the grammar of the film" are phrases one encounters with ever increasing frequency in writings about the cinema. Most often, they occur as metaphors with no real linguistic significance. At the same time, more serious film theoreticians are invoking the names of linguists, sociologists, and structural anthropologists (de Saussure, Hjelmslev, Bernstein, and Levi-Strauss are currently popular) and infusing their writings with adaptations of the theories associated with the names in this litany--for example, Wollen (1969). Actual linguistically oriented work in this area has centered around the semiology of the cinema--most notably in the writings of Christian Metz (1968, 1971) in Europe and Sol Worth (1968, 1969) and Calvin Pryluck (1968) in this country. This paper will concern itself with the application of the linguistic concept of contextualization to the cinema as a methodology towards a semiotics of the cinema.

Although the linguistic use of the term contextualization is generally associated with the theories of J. R. Firth, it first appeared in the writings of the anthropologist Bronislaw Malinowski--in fact, he coined the term context of situation (Malinowski 1923). This expression was to indicate that on one hand the concept of context had to be broadened and that on the other hand the situation in which words are uttered can never be passed over as irrelevant to the linguistic expression.

The idea of context, Malinowski believed, had to be expanded beyond the limits of merely linguistic environment to include an analysis of the conditions under which the language is spoken; and the study of language had to be carried out in conjunction with the study of the speakers' culture and environment. For, he claimed, an utterance "in the reality of a spoken living tongue" has no meaning except in the context of situation.

After contacts with Malinowski in seminars at the University of London in the early 1930's, Firth apparently adapted some of the anthropologist's ideas to his own theory on the context of situation in relation to meaning. In "The Technique of Semantics" (Firth 1935) Firth resolved meaning into five principal component functions: phonetic, lexical, morphological, syntactic, and semantic. Each function was to be considered as being contextualized within the next higher function, from phonetic through semantic, with the contextualization of the semantic function being the context of situation. Thus, meaning for Firth can be described as "a serial contextualization of our facts, context within context, each one being a function, an organ of the bigger context and all contexts finding a place in what may be called the context of culture." (Firth 1935)

Whereas "the context of situation for Malinowski is an ordered series of events considered as in rebus," Firth's view, on the other hand, is that context of situation "is best used as a suitable schematic construct to apply to language events, and that it is a group of related categories at a different level from grammatical categories but rather of the same abstract nature." (Firth 1950)

Within the context of situation he postulates the following interior relations:

1. The relevant features of participants: persons, personalities.
   (a) The verbal action of the participants.
   (b) The non-verbal action of the participants.
2. The relevant objects.
3. The effect of the verbal action. (Firth 1957)

Robins (1964) notes that Firth's categories or interior relations are not exhaustive and that they may be varied in composition as the semantic explanation of the material requires. Though abstract, these categories are applied to the actual environment and "provide the means whereby in each individual case the features relevant to the functioning (meaning) of the utterance and its parts may be singled out for attention." (Robins 1964)

Firth's analysis of the meaning of an utterance, then, involves a study of its interior relationships (of a strictly linguistic nature) and the relation of these in turn to the elements of the context of situation. Such a system has already proven useful in the analysis of certain language varieties--for example, Indian English (Kachru 1965, 1966) and the restricted languages of newspaper headlines (Straumann 1935), near Eastern mercantile language (Mitchell 1957), and the little language of Swift's Journal to Stella (Whitley 1966).

In applying these concepts to an analysis of the cinema we can note, first of all, that most films depict people engaged in speech acts of one sort or another. Each of these speech acts will have its context of situation within the film. But, whether the context and the utterance are completely invented by the screenwriter or the film is a documentary recording of real situations and real utterances, these speech acts should not be considered finally meaningful in themselves but only as one level of contextualization within the context of the film itself as a whole. For the prime communicative event of the cinema is that of the individual film itself, as a whole, in its function as communication between film maker and film viewer.

The meaning of the film, as is the case with the meaning of an utterance, consists in various functions and their contextualization each within the next higher function. It would be overambitious and misleading at this time to attempt to draw exact correspondences between the linguistic functions enumerated in Firth's theory and the various functions operating within the cinema. On the other hand, the functional levels operant within the cinema can be defined phenomenologically, albeit somewhat simplistically, as follows.

Beginning in the middle of this hierarchy, we have the shot, which can provisionally be defined as the apparently uninterrupted recording by the camera of a single event; a film may consist of one

shot (as was the case with the earliest films) or thousands. Contextualized within the shot are two channels through which information passes to the viewer: the visual and the audial. Contextualized within each of these are various codes proper to the channel within which they are contextualized--for example, within the audial channel there are the codes of dialogue, musical score, and sound effects (an exhaustive classification) and within the visual channel there are the codes of lighting key (intensity), camera angle, and distance from camera to object (a non-exhaustive classification). Within these codes--dialogue is the most obvious example--there will be further levels of contextualization proper to the codes themselves.

Returning to the level of the shot, it can be viewed as being contextualized within the sequence, a distinct segment of the film, composed of one or more contiguous related shots (as with the shot in certain cases, it is possible for the sequence to comprise the whole of the film). Sequences, in turn, are contextualized within the individual film itself as a whole (these last two instances of contextualization comprise what most film commentators are referring to when they use the term "syntax of the cinema"). Finally, the film itself is contextualized within its context of situation.

In terms of context of situation, a modification of Firth's categories applies. The interior relations of the context of situation of the cinema can be viewed as follows:

1. The relevant participants: roles and personalities.
2. The relevant objects: genre and place of showing.
3. The effect of the filmic event.

Here relevant participants are film makers and film viewers (or audiences), with film exhibitors forming a secondary category.

With respect to the cinema as communicative event, we can speak of two major groups (or roles) of participants. First, there are all the people (producers, directors, writers, actors, editors, cinematographers, and other production crew members) directly responsible for various aspects of the actual making or production of the film; these, considered as a group, are the film makers. In contributing to the production of a film, the film makers intend (as an end in itself or as a means to some other end, such as earning a living) to communicate certain meanings to another group of people, the film viewers who are exposed to the completed film, which will entertain or instruct the members of this second group.

A third important group, though perhaps not an absolutely essential one, is that of the film exhibitors. This group stands in about the same relation to the other two participatory groups and to the situation as a person performing introductions stands in relation to the persons introduced and to that situation: the function is important, but definitely secondary in its influence on the subsequent language of the other participants.

Other roles exist in the context of situation of the cinema (for example, distributors and advertisers), but they can only be considered as peripheral to our concerns here.

Relevant objects are interpreted in this system as genre and place of showing. Genre refers to the type of material presented in the film--its story classification. The material may be a western,

a love story, a polemic, an educational tract, and so on, or it may embody a combination of genres (a western, for example, may include elements of a love story). Place of showing is closely related to the role of the exhibitor, but it is nonetheless distinct from that feature of context. This distinction becomes explicit when a person or agency not in the practice of exhibiting films is called upon to do so; in this case, both aspects will be important (for example, in an obscenity trial the prosecution may use the film itself as evidence and show it either in the courtroom itself or in a rented theatre). Whether one views a film at a theatre, at home, or in a classroom does have an effect on one's apprehension of the meaning of the film.

Effect of the filmic event should be understood here as the effects intended by the film makers in producing the film as well as the effects actually achieved. This conflation of intent with result allows for aesthetic considerations within the system: where intent and result are at variance and this variance is not due to changes within the other relevant aspects of the context of situation (a case to be discussed later), we can assume that it is due to some lapse of artistry in the handling of the material presented.

Together with the codes embodied within the film these elements of the context form a contextual unit. This concept was developed with respect to language texts in Kachru (1966) where it was described as follows: "A contextual unit is set up in an attempt to demarcate the textually relevant features of a 'situation' in terms of the contextual parameters. It may be viewed as an abstraction on two axes: the syntagmatic and the paradigmatic. Syntagmatically, we think of a contextual unit as having clear endpoints in the time dimension; paradigmatically, it is a bundle of features comprising one or more contextual parameters." Contextual parameters are defined as "variables which determine the effective operation of a text in a contextual unit."

If any element in the context of situation is changed, there must be a corresponding change in the codes employed and consequently a different contextual unit is formed. In the cinema, if there is no corresponding change in the codes used, various sorts of misunderstandings arise due to the resultant anomaly in the contextual unit.

This one of the problems often encountered in dubbing a film into a foreign language. Here, the audience, one group of significant participants, has been changed: the film was made using one language (L_1) for an audience understanding L_1, and the new audience understands only a different language (L_2). A major code is denied the L_2 audience thereby. One solution to this dilemma is to replace the dialogue part of the soundtrack (the audial channel) with another, translating all the dialogue into L_2; but when this is done, a discrepancy is created between the words heard in L_2 and the lip movements of the original L_1 dialogue as well as the accompanying bodily gestures perceived visually by the L_2 audience. In some cases where L_1 and L_2 are relatively closely related (as is the case, for example, with German or Swedish films dubbed into English) this discrepancy will be minimal. With more divergent languages and cultures (Japanese or even Italian dubbed into English) the discrepancy will be more evident and annoying to the L_2 audience.

Similarly, when a modern audience views a film made before the advent of recorded sound, the effect is often far from that intended by its makers. The modern audience is too sophisticated for (or perhaps merely unused to) the codes embodied in the melodramatic acting style and comparatively static directing apparent in many early films. What had evoked pity and fear in its original audiences now only provokes amusement and ridicule. Here again, a major feature of the context of situation has changed without there being a corresponding change in the codes contextualized within the film (text), and this results in a distracting anomaly.

It should also be pointed out that the great mass of literature produced by film critics espousing the American revision of the French auteur theory (and related structuralist theories) depends ultimately on concepts of contextualization. In this case, the emphasis is on the relationship between the individual directors and certain readily identifiable codes they consistently employ in their films which differentiate them from other films of the same genre. This theory, in effect, regards the contextual unit of the film as dependent for its basic character on one feature of the major participants of the context of situation--that is, on personalities, which can be described as the distinguishing features of individuals within the larger groups (or roles) of film makers.

In a recent study of films made by Navajo students, Worth and Adair (1972) used a theory of what they termed "codes in context" in their analysis. Taking a rather unstructured view of context (defining it as "those dimensions which explain some important aspects of the specific situation within which the films were produced") and equating code with the film itself, the researchers identified several visual codes (in the sense of the term as used in the present work) within the films produced by their Navajo students which correlated with features of the Navajo culture and language.

Leskosky (1974) using a conception of context of situation somewhat less elaborated than the present model demonstrated how systematic differences in the context of situation along the lines of personalities of major participants, genre, and effect among three types of film (popular entertainment films, theatrical erotica, and mail-order erotica) were correlated with differences in even the linguistic codes of the film titles. Even where these aspects differed only slightly (in the case of the two types of erotica) differences in exhibitor and place of showing correlated with altered codes in the titles.

The concepts of contextualization, context of situation, and contextual unit, then, can be seen, from the few examples given above, to be applicable to cinema studies. Moreover, such concepts have already figured in some analyses, even though (as with the auteur theory example) they might not have been made explicit or utilized properly. Adoption of a fully developed, explicit use of a theory of contextualization would not only give a formal system to current works on the cinema but would also provide a framework and additional parameters for further research.

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BETWEEN INSIGHT AND EXPLANATION: MUSICAL LINGUISTICS

David Lidov

Department of Music

York University

For some years now the idea of basing musical studies on linguistics has sustained a very respectable ripple in the pacific waters of musicology. Ethnomusicologists have found suggestive models in anthropologically oriented linguistics, musical psychologists in psycholinguistics; musical theorists in generative and distributional syntactics. Yet, music has a fine tradition of formal theory of its own, and I must wonder if we, in music, have done all we might to offer its suggestive influences to linguists. My own interest in musical linguistics centers on the analysis of traditional (Western) music theory and classical music. I would like to explain here where, in my opinion, linguistic thinking can have some dynamic points of contact with classical music theory, points where a methodology, or at least a mentality, influenced by linguistics improves or expands music theory. Hopefully, by exposing and criticizing music theory this way, its formal structure can be made more apparent and more available to linguists.

I would like to explain three concepts of classical music theory-tonality, meter, and phrase - as briefly as I can by applying them to a very short song by J.S.Bach, a composition which we know from the notebooks of his wife Anna Magdalena. Then, comparing these concepts to each other within the perspective of formal grammar theory, we can see how they differ structurally and how they relate to formal models of speech.

The classical theory of tonality tells us that there is a principal tone, which we call the tonic, from and back to which the melody and harmony of a composition move. Fig. 1 is an analysis of the Bach song on the basis of this theory. It depicts a hierarchy of excursions from the tonic center of the piece, which is D in this case. The overall motion is represented by the highest arch (1), which joins the initial and final notes, both D. The next lower arches (2) articulate this motion by interpolating an A. On its way from D to A (2) the music forms a cadence on F (3). Tonality allows for an indefinitely recursive interpolation of ornamental tones between outline tones, each ornament becoming in turn the basis of further elaboration. In general, the interpolated notes must form consecutive scale steps with their neighbors or else form a prominent harmonic relationship with one of them.

Two points about this theory are worth some emphasis here. First, theoretical descriptions of essentially the same structure as Fig. 2 have a precedent at least as far back as the writings of K.P.E. Bach,

son of our composer. They have, historically, a certain authority. Second, there are no formal rules for extracting the analysis from the text (though I know one composer and one linguist who have been working together to write such rules with some success). Musicians can disagree in a case like this about harmonic categories and their hierarchy. This is not to say that the analysis is arbitrary; other musicians can recognize the intuitive logic which lies behind the complicated assumptions and subjective judgements on which the analysis depends.

Rhythmic theory does not comprise as distinct and elaborate a body of ideas as harmony in classical music, but some definite conceptions are inherent in the notation of music. Classical theory characterizes the composition which we are considering as expressing a triple meter. That means it is organized by a regular pulse and that the pulses are grouped into units of three called measures. Classical theory identifies these units, or measures, as hierarchical structures. Each measure has one pulse, called the downbeat or the first beat, which has a special emphasis. We may think of this emphasis as an accent, but it is not an accent which depends on any kind of physical stress. It is an accent which we experience as a result of the total structure of the music. Classical theory does not explain this effect but clearly takes it for granted. 1 Through distributional analysis we can refine and clarify the traditional sense of metrical structure. In our song, there are sixteen measures. Fig. 2 shows the seven different rhythmic patterns used in these 16 measures. The possibility of different rhythms derives from the fact that one pulse can be subdivided into shorter tones and two or more pulses can be combined as a long tone. (Note that in the bass part there is one note per pulse in most of the measures). Looking over the patterns in Fig. 2 we can note that the downbeat is different from the other beats in that it is never subdivided nor unarticulated. It is also the beat of the last note. Such features and/or other codes which govern the use of the downbeat apparently establish its dominance without physical stress (loudness).

While classical theory identifies the measure as a hierarchial structure, it does not recognize any extension of rhythmic hierarchy to larger units. A number of individual thinkers have tried to formulate such an extension, usually by subsuming in rhythm the idea of phrases. However, their ideas have not won the kind of general and intuitive acceptance which musicians accord to the ideas of meter and tonality. We will treat phrase structure as a separate theory, therefore, not as an extension of meter.

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1 The internal hierarchy of the measure is assumed, for example, in the definition of a suspension or a feminine cadence.

The notion of a phrase in classical music is a less formal idea than the ideas of tonality and meter. It is really a different type of idea. Tonality and meter should be considered as abstract systems which music represents concretely. A phrase, on the other hand, is a segment. There is at the same time an abstract idea of regular or balanced phrasing, but the ideal portrayed by the abstract notion of phrasing has only an indirect correspondence with real music, and the dynamics of the relation between real music and "normal" phrase structure are not explained by traditional theory. The essence of the ideal or "normal" structure is a regular doubling of lengths. The period of sixteen measures consists of two eight-measure phrases; these, of two four-measure half-phrases. The half-phrases can be divided into two-measure groups, and these latter are composed of motives or figures more or less associated with the duration of one measure. In the literature of traditional theory there is a considerable variation in the use of these terms.

An interesting and important aspect of these phrase structure principles (which do not apply in equal force to all classical styles) is that despite their symmetry they are not indefinitely recursive. On the contrary they represent fairly distinct strata. The motive, the molecule of the system, is the least expression of a definite image. The phrase or period, at the other end, is thought of as a "complete" unit. Very frequently writers on music will define the phrase by analogy with the sentence of language.

It happens that our particular example, the song by Bach, fits the ideal of phrase structure rather cleanly. Fig. 3 shows the song parsed by the theory of phrase structure. It also shows, by the use of letters as labels, which phrase components are variants of each other. The combination of contrasting, repeating, and varied ideas is the first field of action in musical phrase construction. An ingeniously varied component may resemble two others which are themselves mutually contrasted. However, the phrase parsing need not exhaust these varied repetitions as you can see from the figure which crosses segmental boundaries, marked b* below the staff. 2

Here, then, we have three ways of describing the music, according to its tonality, rhythm and meter, or phrase structure. Does this multi-dimensional approach represent a necessity or an artifice? Ballet, so far as I know, conceptualizes motion without making such a strong division between space and time. The linguistic model would seem to suggest a more unified theory, but the formal structures of tonality, meter and phrasing are quite different and not lent in any obvious way to quick and easy marriage.

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2 In the version I have quoted, the first phrase is repeated. Another texted version shows both phrases repeated. We could also describe the composition as consisting of two periods, each composed of four-measure phrases.

The tonal analysis is a network. Mathematically, nets and trees are equivalent. A topologist can turn a derivation tree into a net by exchanging the nodes and branches, and vice versa. Unfortunately, if the topologist tries this operation on our song, he will have to saw each note in two. What this means is that the tonal analysis does not yield musical segments. As you can see in Fig. 2, the end of one unit is the beginning of the next; so, in the case of a boundary note, we don't know whether to group it with its left neighbor or its right.

The metrical theory gives us distinct segments. Perhaps its segmentation seems trivial if the segments always coincide with the notated measure bars of the music, but there are ways to enrich our concept of meter beyond merely identifying downbeats. 3 Still, the formal characteristic which makes metrical structure so different from tonal structure is that it goes on forever. After each measure comes another. Tonality gives us a beginning and an ending; it provides articulation points but no segmentation. Meter suggests a minimal segmentation but no global framework and no ending.

Phrase structure (Fig. 3) obviously lends itself to notation as a derivation tree. Phrase, as we receive the concept from classical theory, provides both segmentation and a global hierarchy, but it is the least complete of the concepts we have examined. Classical theory does not explain how the different strata of phrase structure come to differ nor how different constituents at the same level relate to each other. Nor, as I said before, does it tell us much about the relation between the ideal, symmetrical form and its varied realizations in actual music. 4

In this area, I think one helpful tool which linguistics offers to musicology is the idea of distinctive features. The idea must be adapted as well as adopted. The distinctive features which characterize phrase components in music may result from locally defined equivalences or from general traits of classical style.

The classical, intuitive sense of phrase responds to a very complex set of relationships. I have been interested in the possibility of paraphrasing these with a matrix of simultaneous positional and durational forms expressed by different systems of distinctive features. For example, we can characterize the four half phrases of

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3 We can include patterns with secondary stresses and patterns which do not start on the downbeat as categories of meter.

4 Not that these problems are virgin territory. Among others, Hugo Riemann, a turn of the century theorist, and Arnold Schonberg, the composer, have offered very informative analyses of these aspects of phrase structure.

Fig. 3 as expressing the positional form (ABBA) where A represents identification with the tonic and B represents the absence of that feature. We could characterize the same four half phrases as expressing the form (ABAB). Here, A represents a complicated feature: In the first half phrase (A...) the third and fourth measure are rhythmically identical. They tend therefore to be perceived as separate units and they give to the half-phrase as a whole the characteristic durational form of Long (two measures) - Short (one measure) - Short (one measure). The third half-phrase (..A.) echoes this structure because the bass voice in measure twelve is the same as the melody in measure eleven. The second and fourth half-phrases (.B.B) lack this structure. A third form (AABB) characterizes the half phrases if B represents the feature C# and A its absence. These descriptions may seem a bit forced and arbitrary, but I hope they comprise a suggestive sample. The phrases and half-phrases have different degrees of completeness and dependence. These are not relationships expressed in any general form by classical theory. Linguistic-like concepts can, I believe, considerably clarify their workings.

The tentative unity of linguistic theory which is suggested (even if not established) by generative formulations seems to have no parallel in music theory. Obviously you can't deduce tonality from meter nor meter from tonality. Perhaps one might envision a generative description based on phrase structure in which tonality and meter realized abstract features. However, most of the world's music does not display the convenient phrase symmetry of our example. If there are any universal kernels for musical phrase structure, they have been hidden with great art.

Music theory has always been unique in the arts because it comes so close to an abstract science. Susanne Langner once wrote that "insight" was an adequate goal for an aesthetic theory rather than proof. Musical analysis offers something more than insight. Its tools are extremely refined and logical, but certainly it falls short of the ideal of proof represented by linguistic explanation. If we regard our theories as generative descriptions, we will find that they generate pure rhythm, pure harmony, or pure phrase. We are nowhere near explaining how composers do all three at once.

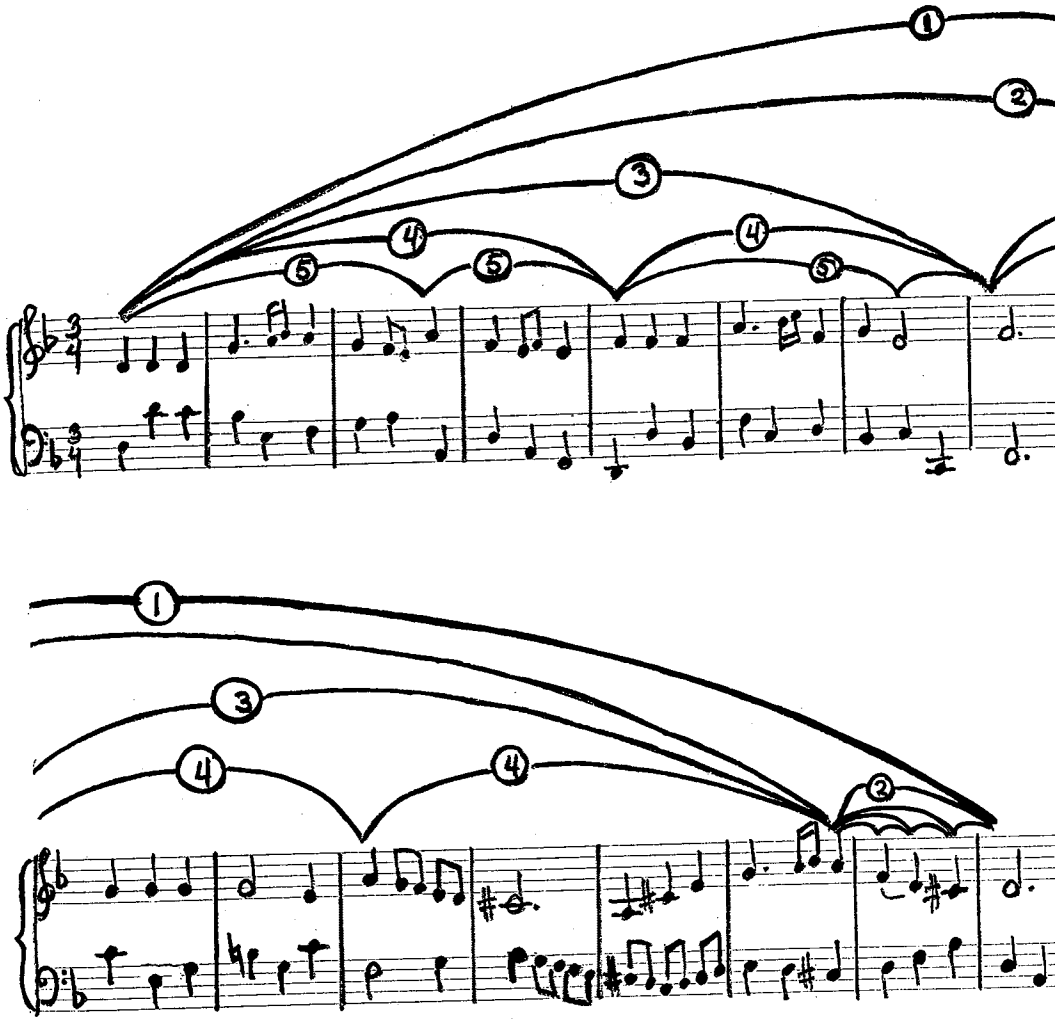


Fig. 1. Tonal Analysis

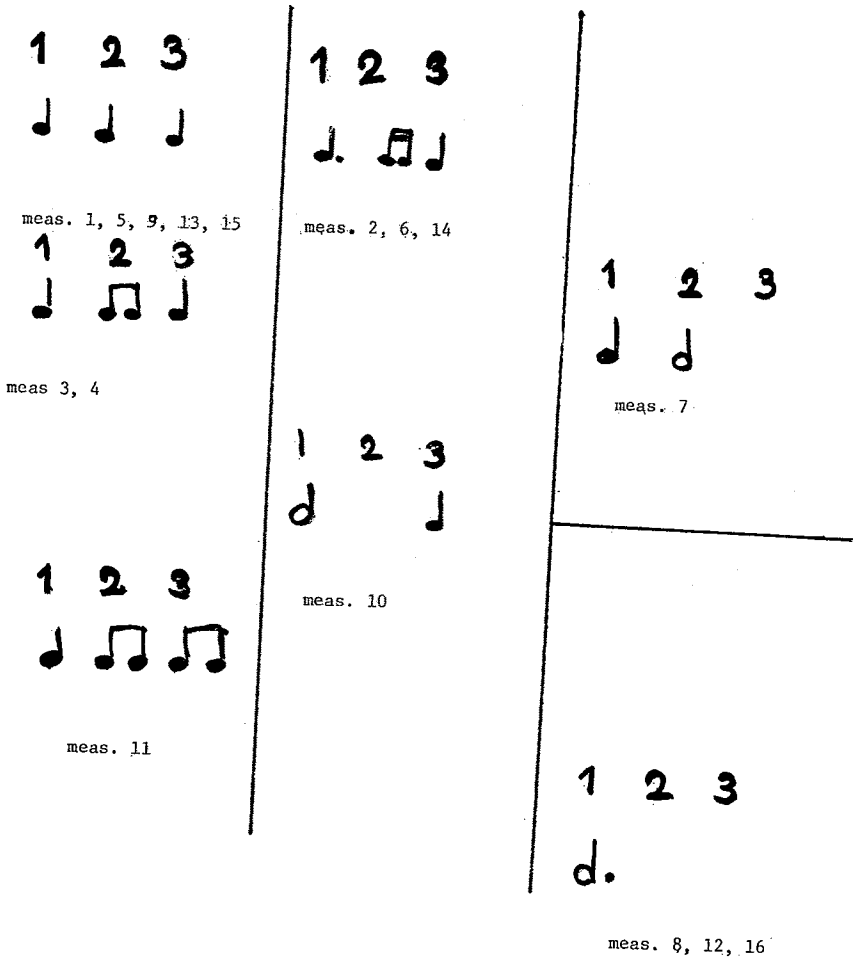


Fig. 2. Distribution of Rhythms

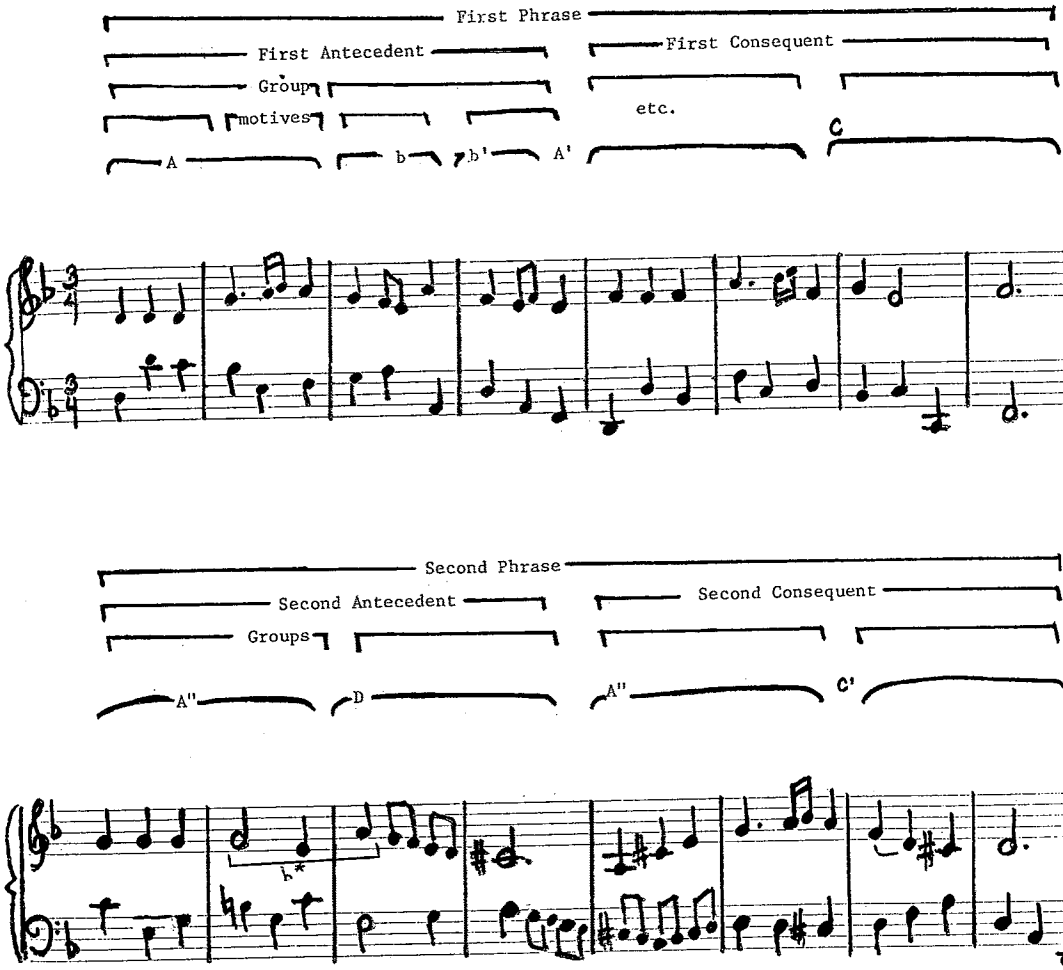


Fig. 3. Phrase Structure and Constituent Repetition (Variation)

THE NON-DICHOTOMY OF SENSORY AND GRAMMATICAL RELATIONSHIPS : TOWARD A RELATIONAL THEORY OF THE BUILT ENVIRONMENT

donald preziosi

mit

0. The title of this paper refers in part to a series of preliminary conclusions regarding the recent observation of deep-structural equivalencies among aspects of verbal language, environmental structure, and various products and processes of cognition.

These observations have grown out of an ongoing inter-disciplinary project begun five years ago, one of whose central aims has been the elaboration of viable methods for the descriptive analysis of the built environment - or what I shall refer to here alternatively as architecture, environmental structure or built form. This work has grown out of a double dissatisfaction - on the one hand, with the apparent inadequacy of currently available analytic methods within this area itself, and on the other hand with the increasing frequency with which writers in this area had begun to develop theoretical frameworks for built form which in nearly every way were literal, and embarrassingly scientistic copies of linguistic paradigms - and in particular, literal copies of the then-dominant transformational-generative (TG) paradigm.

The phenomenon of paradigm-poaching had been a constant feature in studies of the environment since the end of the eighteenth century, and is part of a broader phenomenon which might best be characterized as the result of a pervasive confusion between what would now be called prescriptive and descriptive theory. This confusion has only recently, in the past decade, been resolved (Collins, 1965), although the acceptance of its resolution has been slow.

In a recent paper, Yngve (Yngve, 1975) referred to the tyranny of the concept of language as one of the major obstacles in the way of the elaboration of a more profound understanding of how people communicate; for students of the environment, the concept of "architecture" has been an equally obstinate stumbling block. It has become increasingly clear in recent years that the study of architecture cannot remain self-centered and independent of work in those areas of human knowledge concerned with the multifarious aspects of human communication and interaction. This, then, is the other side of the coin; it is only by seeing the built world as a component in a dynamic, interactive network of human relationships that its unique properties can be isolated.

It is well known that an increasing body of research and writing over the past two decades has been addressed to various aspects of para-linguistic (and para-architectural) behavior. For the most part, this work has focussed upon spatio-kinetic behavior on the part of individuals and groups; results have led to the crystallization of newly-isolated areas of study such as sociolinguistics, proxemics and environmental psychology (see below under Pike, Halliday, Bernstein, Douglas, Piaget, Hall, Somer, Moore, Downs and Stea).

While a great deal of this work has been extremely insightful - in particular that of the structuralist school - much of it has remained trivial in conception and experimental method, a situation which is due not least of all to the non-existence until quite recently of a viable framework for the understanding of the structure and behavior of the built environment itself.

1. It may be useful to paraphrase a remark made by Halliday at this conference last year (Halliday, 1975) in order to focus upon this question: architecture, or environmental structure, is not just an ornamental backdrop or stage set for linguistic and social interaction, as it has tended to become in sociolinguistic and proxemic writing - and, indeed, in formalist architectural history ; rather, built form is an essential element in the evolution of semantic systems and processes in cultural interaction, occupying quite as pervasive a role in such interactions as verbal language itself.

As we have come to understand only recently, the built environment displays underlying properties which in a number of profound ways appear to be equivalent to certain properties of linguistic and other coding systems, both artifactual and organic.

In a number of respects, the subject of environmental structure is a forbidding one: the built world lies squarely in the midst of everything we do as human beings, and of everything we are. Traditional difficulties in the elaboration of adequate descriptive models for the environment have centered upon its very great complexity, the broad range of its transformations of scale and medium, and its highly involved polysemy and multiple referentiality.

The structuring of the environment is a cognitive process achieved through symbolic means, and the use of a language of built form is but one way of achieving this. The origins of the built world may in fact lie in a coordination of perspectives on the ordering or controlling of the environment, and in particular on groups of diverse environmental ordering routines such as calendrical notations, two-dimensional and three-dimensional artifacts, place-markers relevant to seasonal migrations, as well as proto-language.

Hence, rather than seeing "architecture" as an elaboration of originally simpler environmental orderings, we would tend to view it as a simplification of a series of randomly interrelated groups of strategies for the control of natural variation.

Much of the study of architecture has focussed upon an essentially trivial aspect of its diachronic variation - its so-called evolutionary history. It is becoming more and more apparent that such a "history" is largely illusory, and is concerned chiefly with variations in surface organization - namely material and semantic structure. The underlying behavioral properties of environmental structure - what we have come to call its formal syntax - has remained essentially the same as far back as the archaeological records go. The truly profound revolution which gave rise to built form as we know it today must have taken place hundreds of millenia ago; in underlying organization, the mammoth bone shelters of the Ukraine several dozen millenia ago are fundamentally identical to our own urban artifacts.

Whenever such conceptual developments took place, it seems reasonable to assume that they took place prior to or co-occurrent with the development of verbal language in its present form; as we are now beginning to understand, both systems share a number of deep-structural properties. In general, linguistic and environmental orderings differ more radically in the direction of structures of manifestation and realization, whereas structural equivalencies become more pervasive in the direction of deeper or more generalized orderings, as we shall observe below.

2. Since 1970 a group of us, first at Yale and now at MIT, have been involved in an ongoing project whose aims have in part included the elaboration of systematic analytic methods for the study of built form. One of the preliminary results of this work has been the development of a generalized descriptive calculus - what we have come to call a relational calculus - for spatial artifacts. The calculus has grown out of a close study of large bodies of architectural data in specific historical contexts.

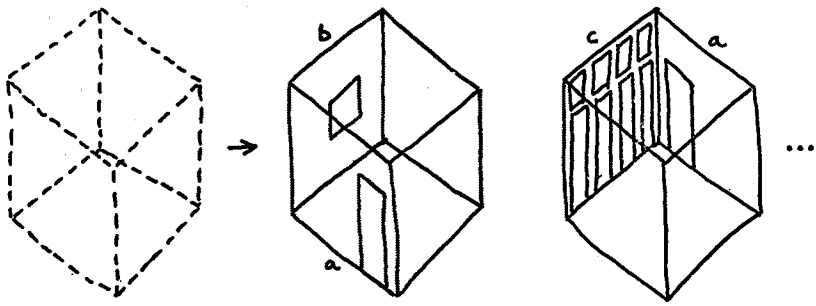
Our broader aims include the relation of this work to similar work in other areas, along with an attempt to clarify our emergent theoretical perspectives within the context of current explanatory models for cognition and perception. We are presently seeking to relate our findings to a spectrum of issues concerned with the nature of informational systems, both organic and artifactual.

We have come to understand architecture as primarily a system of relationships only indirectly observable in the spatial products which serve as immediate data. It was found that a corpus of data could be usefully decomposed into series of component subsystems or frames of reference, each of which exhibited characteristic patterns of relationships.

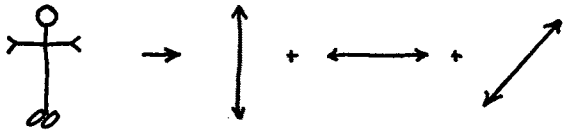
We have seen the chief task of architectural analysis to be the accounting for the occurrence and behavior of built forms in specific contexts. We have employed the term corpus to refer to the entire set of forms potentially manifestable given certain groups of underlying relational properties. It will be clear that the predictive capabilities of an explanatory model will be in part dependent upon the definition of the boundaries of the data at hand. Certain systems (such as kinship systems) comprise structures which are finite, while others (such as linguistic and environmental systems) allow an indefinitely large and unpredictable number of performances. A set of architectural forms is essentially an open set, diachronically and diatopically.

It was found early on in our work that attempts to generate taxonomies of "elements" in a given environmental setting quickly led nowhere (or rather everywhere at once), and demonstrated little more than justifications of individual verbal categorizations - itself interesting information, but not quite what we were after. These early attempts resulted in extensive lists of items or forms which differed from person to person in our group, plus highly complicated and idiosyncratic compositional routines or arrangements.

It became clear that what was needed was some generalized, uniform measure or analytic frame which could be superimposed over portions of an environmental setting, and within which could be observed variations in the occurrence and behavior of isolable components. Such components as might be isolated through such a process could then be described solely in terms of their contrastive relations with each other:

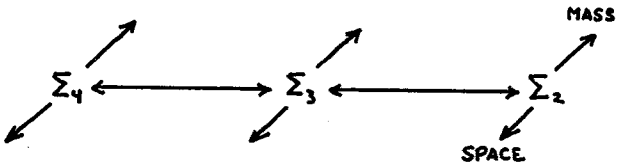


Since we were dealing with a tri-dimensional space frame, such variations could be seen in terms of contrasts of relative ratios among height, length and width. The framework itself is essentially a simple projection of the three-fold bilateral geometry of the human form itself:



We admitted to a corpus only such forms as entered into distinctive contrasts, ignoring momentarily wide variations in materials and functional reference. "Items" or forms which stood in similar positions in the space-frame were then considered equivalent in that they were interchangeable in this context of the corpus. By "equivalence" was understood that by a process of continuous transformation it would be possible to convert any one of such isolable entities into any other simply by readjusting relative ratios of height, length and width.

It was later discovered through the study of ambiguities in the class of isolable entities that architecture in fact employs a tripartite referential frame (in contrast to the single dichotomous scale of language), involving classes of relational properties in three-dimensional euclidean space, topological space, and space-time (3-space, 2-space and 4-space). This tripartite space-frame is further distinguished by a binary contrast in each stratum between properties of mass and properties of space:



By this process it was possible to isolate what turned out to be a surprisingly small number of contrastive "forms" - themselves comprised essentially of bundles of relational properties. Corpora were seen to contrast both in terms of the identity of underlying formal properties ("entities") and in terms of the relational operations among formal entities. Certain relationships were permissable in a given corpus, while others were not:

We came to call this overall class of relational properties in a corpus its formal syntax or formal structure, contrasting this with more highly ordered classes of organizational properties concerning, on the one hand, relationships between formal structure and physical form, and, on the other hand, both formal and material structure and networks of cultural experience. It has become convenient to refer to the former as material syntax or material structure, and the latter as semantic structure.

What eventually emerged was an essentially tristratal model for the organization of environmental artifacts, comprising a class of more generalized orderings - formal syntax or deep structure - and two classes of structures of manifestation or realization - material structure on the one hand and semantic structure on the other.

It was observed that isolable formal entities were mapped or projected onto inventories of materials which themselves were physically realized within the framework of complex (and corpus-specific) subroutines - what we have come to call modularization rules - involving relationships of absolute size, color, texture, and so forth.

Semantic structure was seen as a group of orderings involving the mapping of isolable formal and material entities upon ordered networks or reticula providing reifications of various aspects of cultural experience. We observed semantic relationships in architectural artifacts which broadly speaking could be seen as equivalent to linguistic relations of synonymy, hyponymy, and polysemy. An example of a semantic relationship in architecture might be an equivalency made between, say, the layout of a village and the topography of the human body, or an equivalency made between the form of a domestic structure and a network of macrocosmic relationships.

3. It was also found that relationships among entities both within organizational strata and between strata were not only homologous but were also definable in terms of five basic relational operations:

(1)	conjunction	($\cup$)	(both/and)
(2)	disjunction	($\cap$)	(either/or)
(3)	negation	($\neg$)	
(4)	implication	($\supset$)	
(5)	equivalence	($\equiv$)	

We originally portrayed the interactions among relational entities in linear fashion, in the language of logical symbols, as it had become apparent that the internal syntactic relations of environmental structure obeyed a binary logic akin to that of a simple Boolean propositional algebra.

As we had come to understand more of the complexities of environmental syntax, however, the diagrammatic space of our

analytic model had become increasingly overgrown, making analysis more and more cumbersome. It was clear that we needed to develop a notational system which could rapidly and efficiently portray relationships observable among isolable properties, and which could also preserve something of the multidimensional character of the emergent conceptual space of our theoretical model.

It seemed that some modified version of a switching algebra might be of use - something which might preserve the peculiar geometries of our model; we found that a slightly altered version of the relational system employed by Lamb in the stratificational theory of verbal language (Lamb, 1962) was well suited to our purposes, although the resultant diagrammatic spaces in each will be seen to contrast sharply in detail (Preziosi, 1974b; 1975). We have been employing such a system since 1971; in the intervening years it has undergone its own idiosyncratic development.

4. A great deal has been happening in the study of the built environment in the past half decade; it would be a task well beyond the space of this report to attempt to present a comprehensive account of this work and all its emergent implications. It will be clear, however, that in this extremely rapid overview we have touched on a number of areas which suggest possible correspondences with portions of a number of structuralist models for verbal language.

It may be useful at this point to enumerate such areas within linguistic and environmental structure which our work in the latter has come to suggest equivalent behavioral processes. We distinguish seven broad areas of equivalency:

(1) the manner (but not the identity) of the formation of distinctive features;

(2) the broad geometries of the relational interactions among distinctive features;

(3) the phenomenon of the generation of a transfinitely large body of gross forms out of a small stock of relational entities;

(4) the nature of equivalency and invariance in both;

(5) the existence of a property of duality or semi-autonomy pervasive in both systems - alternatively, a property of conditional elementarity in each, whereby the notion of "element" is strictly a function of the perspective one is looking from;

(6) the existence in both systems of isolable organizational strata composed of classes of items defined by relationships of equivalency;

(7) the phenomenon of projection, mapping or realization.

Among the most pervasive properties observable in artifactual and organic systems is the property of duality or semi-autonomy - namely, the partial independence between formal structure and meaning, function or behavior. This property is manifest in (1) molecular interactions, where an independence is seen between a function and the nature of the chemical signals controlling it; (2) in cognitive structure, where identical neural activities are multiply connected to distinct muscular products and processes; (3) in perceptual activity, where distinct stimuli interact with the same sensory structures (and vice-versa); (4) in verbal language, where relations of polysemy and synonymy are pervasive; and (5) in environmental form, where equivalent relationships are pervasive within and between organizational strata.

In both language and built form may be observed a property of conditional elementarity, wherein the identity of isolable entities is principally a function of hierarchical perspective. In both systems, information is preserved in the connectivities of the system; at base we are dealing with relationships among relationships.

5. It seems clear that the logic of propositional operations, whether linguistic or environmental, is not built up in the individual by any direct reference to the logic of neural interactions, however isomorphic such a logic may turn out to be. This would appear to be suggested in the recent work of Lamb (Lamb, 1975) by the continuing clarification of the nature of the stratal boundary between the linguistic system and the conceptual system. It seems more likely that this process involves an uninterrupted series of constructions and equilibrations which may have been oriented by structures inherent in neural functioning but which nevertheless presuppose a new series of instruments.

At its deepest levels, architecture suggests a series of performances or actions perfomed in space with their own somatic relational geometries. In the course of our recent work, we have suggested (Preziosi, 1975) that the tripartite deep-structure of built form is homologous to the stages of successive equilibrations in the learning of spatial concepts by the child. As Piaget and others have demonstrated, the human individual employs a complex, combinatory system of spatial concepts which offers a multiple perspective on external stimuli. The built world is so structured as to address itself to this multiply equilibrated and coordinated perceptual system: equivalent structures of organization (topological, perspectival and euclidean) lie at the core of the formal syntax of built form.

Hence, the built form equivalent to the linguistic "dichotomous scale" is significantly more complex, and addressed to underlying perceptual structures whose organization is tripartite.

One of the consequences of this - and here emerges a significant contrast between linguistic and environmental structure - is that the order of encoding and decoding is rather more free in architecture than in language. A misapprehension of this fact lies at the core of the earlier scientistic paradigms for architecture based largely on the old TG paradigm. We have come to feel that our work on the problems of environmental structure suggests a number of conclusions regarding the nature of informational systems whose closest equivalencies appear to be implicit principally in the relational, stratificational model for language suggested in part by Hjemselv and elaborated by Lamb and others.

These developments came about rather independently; it was not until relatively recently, after the major bulk of our relational theory of built form had been elaborated, that the possibilities of a dialogue became apparent.

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